

thing like an adequate staff, it is inevitable that something must be left undone. It is certainly the first duty of a curator to take care of his specimens, and thus it naturally happens that what is left to a more convenient season is the educational arrangement and descriptive labelling of the specimens; as it is these deficiencies that cause museums to be condemned as uninteresting or uninteresting, so it is difficult to get out of that vicious circle which is so well described by the proverb, "the destruction of the poor is their poverty."

A circular, signed by the Disney professor of archaeology and the curator of the Museum of Archaeology and Ethnology at Cambridge, has been issued directing attention to the congested state of the museum and the inability even to store the existing specimens; the nineteenth annual report of the antiquarian committee, which accompanies the appeal, gives a long list of additions to the museum for the year 1903, which proves that it is rapidly and symmetrically growing. Valuable collections have to be stored out of sight, and so are unavailable for purposes of study. The university has assigned a fine site for the proposed new museum, but as the subscriptions hitherto raised only amount to about 3300*l.*, no steps can be taken towards erecting the building.

Not only do the collections require space and cases for them to be seen by the public, and to enable them to be used for purposes of instruction and research, but rooms are required for the teaching staff, and where research and demonstrations can be carried on. The present teaching staff, of subjects connected with the museum, consists of one professor and a lecturer, both with absurdly small stipends. The circular estimates that for the proper development of the department a new museum must be provided, at a cost of 25,000*l.*, in addition to an adequate annual income for maintenance and for the increase of the stipends of the curatorial and teaching staff.

The circular concludes by pointing out that no better centre than the University of Cambridge can be found for the study of anthropology or for the development of a museum of the best kind; many of her students are led for purposes of research, or in the discharge of professional duties, or for pleasure, to divers quarters of the globe, and not a few among these have enriched the museum with valuable collections. The opportunities for the study of primitive society, and for the formation of collections illustrative of its various phases, are rapidly vanishing before the advance of European civilisation. The funds of the university have been strained to their utmost of late years to keep even the older scientific departments abreast of the times. It is therefore necessary to appeal for outside help in order to raise the funds required for the erection, equipment, and endowment of a museum of anthropology which shall be worthy of the university.

BRITISH ASSOCIATION MEETING AT CAMBRIDGE.

IN the issues of NATURE for July 21 and August 4, articles giving general accounts of the local arrangements and of the main items in the sectional programmes were published. At the time of writing this article the sectional committees had not met, so that the programme of technical papers to be read before the sections cannot be fully announced. The meetings begin to-day, but already the reception room at the Guildhall has been opened, and a very large number of members have applied for reserved seats at the first general meeting, when the president will deliver his address. An exceptionally large num-

ber of tickets have already been sold, so that there is every probability that the Cambridge meeting will see one of the largest attendances the Association has known during recent years. The unusual number of foreign guests who will be present, and the many leading men of science of Great Britain who have accepted invitations will make the meeting a thoroughly representative one in all branches of science. An interesting memento of the meeting is a book of lithographed signatures of the members of the Association who were present at the first meeting in Cambridge in 1833. There are only a few of these books, and they will be on sale in the reception room during the present meeting.

The arrangements of the reception room and general rooms at the Guildhall are very complete, and now that the somewhat unexpected rush at opening is over the attendants will be able to cope easily with the large amount of business that is to be done. A word should be said about the postal arrangements. A temporary post office has been established in the general reception room, where all postal business can be transacted. For the convenience of members a special box has been provided in which notes for members of the Association may be placed unstamped; these will be sorted and delivered with ordinary letters at the post office in the reception room. One of the new features of the general arrangements is the establishment of a Press Bureau. At this office information will be collected from sectional secretaries and will be available for the Press, so that full information can be obtained without the difficulty of finding the sectional secretaries. It is hoped that this arrangement will facilitate reports of sectional and other meetings, and lead to a more satisfactory account of the Association's proceedings in the Press.

A weather forecast will be supplied by Dr. Shaw from the Meteorological Office twice a day during the meeting. This will be posted in the general reception room.

At present we can only give the titles of a few of the papers which have not appeared in earlier articles. In Section A it is expected that there will be a discussion on *n*-rays. Prof. Lummer and probably Prof. Rubens will take part in the discussion of this most debated question. Mr. Burke, who is one of the few Englishmen who have made experiments on these rays, is also expected to contribute to the discussion. Dr. Rotch, the director of the Blue Hill Observatory, is to read a paper on the temperature of air in cyclones and anticyclones as shown by kite-flights at Blue Hill Observatory, U.S.A. Prof. W. Wien will read a paper on experiments to determine whether the ether moves with the earth or not.

In Section A this year is included as a subsection the department of cosmical physics. Under this subsection is the committee appointed by the International Meteorological Committee at Southport in 1903 to combine and discuss meteorological observations from the point of view of their relations with solar physics. The members of this committee who will attend are, so far as is known at present, Sir J. Eliot, Sir Norman Lockyer, M. A. Angot, Prof. Riccò, Prof. Knut Ångström, Prof. Birkeland, Dr. W. J. S. Lockyer, Dr. W. N. Shaw, Mr. Axsel S. Steen, and Prof. S. P. Langley.

In Section B twenty-nine papers are to be communicated. Eleven of these papers are by Cambridge chemists, and a most interesting meeting is looked forward to. The greatest interest perhaps centres round the paper by Dr. Lowry on dynamic isomerism, and the discussion of the report to be presented by Mr. H. O. Jones on the stereo-

chemistry of nitrogen. In this discussion it is expected that Prof. Aschan, of Helsingfors, Prof. Pope, Prof. Kipping, and Prof. Wedekind will take part. In connection with this section the apparatus of Messrs. Heycock and Neville will be on view, and in the University Chemical Laboratory Prof. Liveing has set up an echelon spectroscope with which the Zeeman effect and other interesting phenomena in spectroscopy will be exhibited.

In Section I, devoted to physiology, in addition to the communications already announced, Mr. Hankin, pathologist at Agra, will on Saturday morning deliver a lecture on the spread of plague. Thursday afternoon will be devoted to the heart. Communications from Prof. Sherrington and Miss Sowton on the action of chloroform on the heart, from Dr. W. E. Dixon on the action of alcohol on the heart, and from Dr. G. A. Gibson on the disturbance of cardiac rhythm will be made. Tuesday will be devoted to physiological chemistry. Prof. Macallum, of Toronto, will read a paper on the distribution of potassium in animal and vegetable cells, and a communication will be received from Prof. Kossel and Mr. Dakin on protamines and a general discussion on the nature of proteids. On Friday and Saturday afternoons this section will hold no meetings, but the Physiological and Psychological Societies will meet on these days, so that members of the Association who desire can attend these meetings without missing any of the papers communicated to the Association. On Monday afternoon Prof. Schäfer, of Edinburgh, will give an account of methods of artificial respiration with a special view to the restoration of the apparently drowned. This should be a specially interesting demonstration in view of Prof. Schäfer's new method for producing artificial respiration. The rest of Monday afternoon will be devoted to other demonstrations. The physiological laboratory will be open for inspection by members of the Association during the meetings.

In the Cavendish Laboratory an exhibition of apparatus and objects of scientific interest will be open during the session. Of special interest is the exhibition of geometrical models under Section A, and of models made at various schools under the education section. Among the more interesting of the models under Section A may be mentioned a plaster model of the general cubic surface with its twenty-seven lines drawn on it; and a model of Sir Robert Ball's cylindroid. The Cambridge Scientific Instrument Company is exhibiting a collection of scientific instruments, among which an oscillograph will be shown in action at certain times during the session. The Cambridge University Press is exhibiting a large number of books. Mr. C. E. S. Phillips will exhibit a new automatic vacuum pump, and Prof. J. A. Fleming will show an instrument for measuring wave-lengths used in wireless telegraphy.

Dr. W. N. Shaw will show the "microbarograph" which he and Mr. Dines have recently invented. This instrument is for measuring and recording small and rapid variations of atmospheric pressure, while slow changes are allowed to escape. Various forms of self-recording meteorological instruments will be shown by Messrs. Lander and Smith. A temperature recording instrument is set up at the entrance to the Guildhall by the Cambridge Scientific Instrument Company. In addition to the room used for the main part of the exhibition, the Cavendish and other laboratories will be open for inspection during the session, where members can see the ordinary apparatus in use and study the methods of scientific teaching adopted in the university.

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INAUGURAL ADDRESS BY THE RIGHT HON. A. J. BALFOUR, D.C.L., LL.D., M.P., F.R.S., CHANCELLOR OF THE UNIVERSITY OF EDINBURGH, PRESIDENT OF THE ASSOCIATION.

Reflections suggested by the New Theory of Matter.

THE meetings of this great Society have for the most part been held in crowded centres of population, where our surroundings never permit us to forget; were such forgetfulness in any case possible, how close is the tie that binds modern science to modern industry, the abstract researches of the student to the labours of the inventor and the mechanic. This, no doubt, is as it should be. The interdependence of theory and practice cannot be ignored without inflicting injury on both; and he is but a poor friend to either who undervalues their mutual cooperation.

Yet, after all, since the British Association exists for the advancement of science, it is well that now and again we should choose our place of gathering in some spot where science rather than its applications, knowledge, not utility, are the ends to which research is primarily directed.

If this be so, surely no happier selection could have been made than the quiet courts of this ancient University. For here, if anywhere, we tread the classic ground of physical discovery. Here, if anywhere, those who hold that physics is the true *Scientia Scientiarum*, the root of all the sciences which deal with inanimate nature, should feel themselves at home. For, unless I am led astray by too partial an affection for my own University, there is nowhere to be found, in any corner of the world, a spot with which have been connected, either by their training in youth, or by the labours of their maturer years, so many men eminent as the originators of new and fruitful physical conceptions. I say nothing of Bacon, the eloquent prophet of a new era; nor of Darwin, the Copernicus of Biology; for my subject to-day is not the contributions of Cambridge to the general growth of scientific knowledge. I am concerned rather with the illustrious line of physicists who have learned or taught within a few hundred yards of this building—a line stretching from Newton in the seventeenth century, through Cavendish in the eighteenth, through Young, Stokes, Maxwell, in the nineteenth, through Kelvin, who embodies an epoch in himself, down to Rayleigh, Larmor, J. J. Thomson, and the scientific school centred in the Cavendish laboratory, whose physical speculations bid fair to render the closing years of the old century and the opening years of the new as notable as the greatest which have preceded them.

Now what is the task which these men, and their illustrious fellow-labourers out of all lands, have set themselves to accomplish? To what end led these "new and fruitful physical conceptions" to which I have just referred? It is often described as the discovery of the "laws connecting phenomena." But this is certainly a misleading, and in my opinion a very inadequate, account of the subject. To begin with, it is not only inconvenient, but confusing, to describe as "phenomena" things which do not appear, which never have appeared, and which never can appear, to beings so poorly provided as ourselves with the apparatus of sense perception. But apart from this, which is a linguistic error too deeply rooted to be easily exterminated, is it not most inaccurate in substance to say that a knowledge of Nature's laws is all we seek when investigating Nature? The physicist looks for something more than what, by any stretch of language, can be described as "co-existences" and "sequences" between so-called "phenomena." He seeks for something deeper than the laws connecting possible objects of experience. His object is physical reality: a reality which may or may not be capable of direct perception; a reality which is in any case independent of it; a reality which constitutes the permanent mechanism of that physical universe with which our immediate empirical connection is so slight and so deceptive. That such a reality exists, though philosophers have doubted, is the unalterable faith of science; and were that faith *per impossibile* to perish under the assaults of critical speculation, science, as men of science usually conceive it, would perish likewise.

If this be so, if one of the tasks of science, and more particularly of physics, is to frame a conception of the physical universe in its inner reality, then any attempt to

compare the different modes in which, at different epochs of scientific development, this intellectual picture has been drawn cannot fail to suggest questions of the deepest interest. True, I am precluded from dealing with such of these questions as are purely philosophical by the character of this occasion; and with such of them as are purely scientific by my own incompetence. But some there may be sufficiently near the dividing line to induce the specialists who rule by right on either side of it to view with forgiving eyes any trespasses into their legitimate domain which I may be tempted, during the next few minutes, to commit.

Let me, then, endeavour to compare the outlines of two such pictures, of which the first may be taken to represent the views prevalent towards the end of the eighteenth century; a little more than a hundred years from the publication of Newton's "*Principia*," and, roughly speaking, about midway between that epoch-making date and the present moment. I suppose that if at that period the average man of science had been asked to sketch his general conception of the physical universe, he would probably have said that it essentially consisted of various sorts of ponderable matter, scattered in different combinations through space, exhibiting most varied aspects under the influence of chemical affinity and temperature, but through every metamorphosis obedient to the laws of motion, always retaining its mass unchanged, and exercising at all distances a force of attraction on other material masses, according to a simple law. To this ponderable matter he would (in spite of Rumford) have probably added the so-called "imponderable" heat, then often ranked among the elements; together with the two "electrical fluids," and the corpuscular emanations supposed to constitute light.

In the universe as thus conceived, the most important form of action between its constituents was action at a distance; the principle of the conservation of energy was, in any general form, undreamed of; electricity and magnetism, though already the subjects of important investigation, played no great part in the Whole of things; nor was a diffused ether required to complete the machinery of the universe.

Within a few months, however, of the date assigned for these deliverances of our hypothetical physicist came an addition to this general conception of the world, destined profoundly to modify it. About a hundred years ago Young opened, or re-opened, the great controversy which finally established the undulatory theory of light, and with it a belief in an interstellar medium by which undulations could be conveyed. But this discovery involved much more than the substitution of a theory of light which was consistent with the facts for one which was not; since here was the first authentic introduction¹ into the scientific world-picture of a new and prodigious constituent—a constituent which has altered, and is still altering, the whole balance (so to speak) of the composition. Unending space, thinly strewn with suns and satellites, made or in the making, supplied sufficient material for the mechanism of the heavens as conceived by Laplace. Unending space filled with a continuous medium was a very different affair, and gave promise of strange developments. It could not be supposed that the ether, if its reality were once admitted, existed only to convey through interstellar regions the vibrations which happen to stimulate the optic nerve of man. Invented originally to fulfil this function, to this it could never be confined. And accordingly, as everyone now knows, things which, from the point of view of sense perception, are as distinct as light and radiant heat, and things to which sense perception makes no response, like the electric waves of wireless telegraphy,² intrinsically differ, not in kind, but in magnitude alone.

This, however, is not all, nor nearly all. If we jump over the century which separates 1804 from 1904, and attempt to give in outline the world-picture as it now presents itself to some leaders of contemporary speculation, we shall find that in the interval it has been modified, not merely by such far-reaching discoveries as the atomic and molecular composition of ordinary matter, the kinetic theory of gases, and the laws of the conservation and dissipation

of energy, but by the more and more important part which electricity and the ether occupy in any representation of ultimate physical reality.

Electricity was no more to the natural philosophers in the year 1700 than the hidden cause of an insignificant phenomenon.¹ It was known, and had long been known, that such things as amber and glass could be made to attract light objects brought into their neighbourhood; yet it was about fifty years before the effects of electricity were perceived in the thunderstorm. It was about 100 years before it was detected in the form of a current. It was about 120 years before it was connected with magnetism; about 170 years before it was connected with light and ethereal radiation.

But to-day there are those who regard gross matter, the matter of everyday experience, as the mere appearance of which electricity is the physical basis; who think that the elementary atom of the chemist, itself far beyond the limits of direct perception, is but a connected system of monads or sub-atoms which are not electrified matter, but are electricity itself; that these systems differ in the number of monads which they contain, in their arrangement, and in their motion relative to each other and to the ether; that on these differences, and on these differences alone, depend the various qualities of what have hitherto been regarded as indivisible and elementary atoms; and that while in most cases these atomic systems may maintain their equilibrium for periods which, compared with such astronomical processes as the cooling of a sun, may seem almost eternal, they are not less obedient to the law of change than the everlasting heavens themselves.

But if gross matter be a grouping of atoms, and if atoms be systems of electrical monads, what are these electrical monads? It may be that, as Prof. Larmor has suggested, they are but a modification of the universal ether, a modification roughly comparable to a knot in a medium which is inextensible, incompressible and continuous. But whether this final unification be accepted or not, it is certain that these monads cannot be considered apart from the ether. It is on their interaction with the ether that their qualities depend; and without the ether an electric theory of matter is impossible.

Surely we have here a very extraordinary revolution. Two centuries ago electricity seemed but a scientific toy. It is now thought by many to constitute the reality of which matter is but the sensible expression. It is but a century ago that the title of an ether to a place among the constituents of the universe was authentically established. It seems possible now that it may be the stuff out of which that universe is wholly built. Nor are the collateral inferences associated with this view of the physical world less surprising. It used, for example, to be thought that mass was an original property of matter, neither capable of explanation nor requiring it; in its nature essentially unchangeable, suffering neither augmentation nor diminution under the stress of any forces to which it could be subjected; unalterably attached to, or identified with, each material fragment, howsoever much that fragment might vary in its appearance, its bulk, its chemical or its physical condition.

But if the new theories be accepted these views must be revised. Mass is not only explicable, it is actually explained. So far from being an attribute of matter considered in itself, it is due, as I have said, to the relation between the electrical monads of which matter is composed and the ether in which they are bathed. So far from being unchangeable, it changes, when moving at very high speeds, with every change in its velocity.

Perhaps, however, the most impressive alteration in our picture of the universe required by these new theories is to be sought in a different direction. We have all, I suppose, been interested in the generally accepted views as to the origin and development of suns with their dependent planetary systems; and the gradual dissipation of the energy which during this process of concentration has largely taken the form of light and radiant heat. Follow out the theory to its obvious conclusions, and it becomes plain that the stars now visibly incandescent are those in mid-journey between the nebulae from which they sprang and the frozer

¹ The hypothesis of an ether was, of course, not new. But before Young and Fresnel it cannot be said to have been established.

² First known through the theoretical work of Maxwell and the experiments of Herz.

¹ The modern history of electricity begins with Gilbert, but I have throughout confined my observations to the post-Newtonian period.

darkness to which they are predestined. What, then, are we to think of the invisible multitude of the heavenly bodies in which this process has been already completed? According to the ordinary view, we should suppose them to be in a state where all possibilities of internal movement were exhausted. At the temperature of interstellar space their constituent elements would be solid and inert; chemical action and molecular movement would be alike impossible, and their exhausted energy could obtain no replenishment unless they were suddenly rejuvenated by some celestial collision, or travelled into other regions warmed by newer suns.

This view must, however, be profoundly modified if we accept the electric theory of matter. We can then no longer hold that if the internal energy of a sun were as far as possible converted into heat either by its contraction under the stress of gravitation or by chemical reactions between its elements, or by any other inter-atomic force; and that, were the heat so generated to be dissipated, as in time it must be, through infinite space, its whole energy would be exhausted. On the contrary, the amount thus lost would be absolutely insignificant compared with what remained stored up within the separate atoms. The system in its corporate capacity would become bankrupt—the wealth of its individual constituents would be scarcely diminished. They would lie side by side, without movement, without chemical affinity; yet each one, howsoever inert in its external relations, the theatre of violent motions, and of powerful internal forces.

Or, put the same thought in another form. When the sudden appearance of some new star in the telescopic field gives notice to the astronomer that he, and perhaps, in the whole universe, he alone, is witnessing the conflagration of a world, the tremendous forces by which this far-off tragedy is being accomplished must surely move his awe. Yet not only would the members of each separate atomic system pursue their relative course unchanged, while the atoms themselves were thus riven violently apart in flaming vapour, but the forces by which such a world is shattered are really negligible compared with those by which each atom of it is held together.

In common, therefore, with all other living things, we seem to be practically concerned chiefly with the feeble forces of Nature, and with energy in its least powerful manifestations. Chemical affinity and cohesion are on this theory no more than the slight residual effects of the internal electrical forces which keep the atom in being. Gravitation, though it be the shaping force which concentrates nebulae into organised systems of suns and satellites, is trifling compared with the attractions and repulsions with which we are familiar between electrically charged bodies; while these again sink into insignificance beside the attractions and repulsions between the electric monads themselves. The irregular molecular movements which constitute heat, on which the very possibility of organic life seems absolutely to hang, and in whose transformations applied science is at present so largely concerned, cannot rival the kinetic energy stored within the molecules themselves. This prodigious mechanism seems outside the range of our immediate interests. We live, so to speak, merely on its fringe. It has for us no promise of utilitarian value. It will not drive our mills; we cannot harness it to our trains. Yet not less on that account does it stir the intellectual imagination. The starry heavens have from time immemorial moved the worship or the wonder of mankind. But if the dust beneath our feet be indeed compounded of innumerable systems, whose elements are ever in the most rapid motion, yet retain through uncounted ages their equilibrium unshaken, we can hardly deny that the marvels we directly see are not more worthy of admiration than those which recent discoveries have enabled us dimly to surmise.

Now, whether the main outlines of the world-picture which I have just imperfectly presented to you be destined to survive, or whether in their turn they are to be obliterated by some new drawing on the scientific palimpsest, all will, I think, admit that so bold an attempt to unify physical nature excites feelings of the most acute intellectual gratification. The satisfaction it gives is almost æsthetic in its intensity and quality. We feel the same sort of pleasurable shock as when from the crest of some melancholy pass we first see far below us the sudden glories of plain, river, and

mountain. Whether this vehement sentiment in favour of a simple universe has any theoretical justification I will not venture to pronounce. There is no *a priori* reason that I know of for expecting that the material world should be a modification of a single medium, rather than a composite structure built out of sixty or seventy elementary substances, eternal and eternally different. Why, then, should we feel content with the first hypothesis and not with the second? Yet so it is. Men of science have always been restive under the multiplication of entities. They have eagerly noted any sign that the chemical atom was composite, and that the different chemical elements had a common origin. Nor, for my part, do I think such instincts should be ignored. John Mill, if I rightly remember, was contemptuous of those who saw any difficulty in accepting the doctrine of "action at a distance." So far as observation and experiment can tell us, bodies *do* actually influence each other at a distance. And why should they not? Why seek to go behind experience in obedience to some *a priori* sentiment for which no argument can be adduced? So reasoned Mill, and to his reasoning I have no reply. Nevertheless, we cannot forget that it was to Faraday's obstinate disbelief in "action at a distance" that we owe some of the crucial discoveries on which both our electric industries and the electric theory of matter are ultimately founded; while at this very moment physicists, however baffled in the quest for an explanation of gravity, refuse altogether to content themselves with the belief, so satisfying to Mill, that it is a simple and inexplicable property of masses acting on each other across space.

These obscure intimations about the nature of reality deserve, I think, more attention than has yet been given to them. That they exist is certain; that they modify the indifferent impartiality of pure empiricism can hardly be denied. The common notion that he who would search out the secrets of Nature must humbly wait on experience, obedient to its slightest hint, is but partly true. This may be his ordinary attitude; but now and again it happens that observation and experiment are not treated as guides to be meekly followed, but as witnesses to be broken down in cross-examination. Their plain message is disbelieved, and the investigating judge does not pause until a confession in harmony with his preconceived ideas has, if possible, been wrung from their reluctant evidence.

This proceeding needs neither explanation nor defence in those cases where there is an apparent contradiction between the utterances of experience in different connections. Such contradictions must of course be reconciled, and science cannot rest until the reconciliation is effected. The difficulty really arises when experience apparently says one thing and scientific instinct persists in saying another. Two such cases I have already mentioned; others will easily be found by those who care to seek. What is the origin of this instinct, and what its value; whether it be a mere prejudice to be brushed aside, or a clue which no wise man would disdain to follow, I cannot now discuss. For other questions there are, not new, yet raised in an acute form by these most modern views of matter, on which I would ask your indulgent attention for yet a few moments.

That these new views diverge violently from those suggested by ordinary observation is plain enough. No scientific education is likely to make us, in our unreflective moments, regard the solid earth on which we stand, or the organised bodies with which our terrestrial fate is so intimately bound up, as consisting wholly of electric monads very sparsely scattered through the spaces which these fragments of matter are, by a violent metaphor, described as "occupying." Not less plain is it that an almost equal divergence is to be found between these new theories and that modification of the common-sense view of matter with which science has in the main been content to work.

What was this modification of common sense? It is roughly indicated by an old philosophic distinction drawn between what were called the "primary" and the "secondary" qualities of matter. The primary qualities, such as shape and mass, were supposed to possess an existence quite independent of the observer; and so far the theory agreed with common sense. The secondary qualities, on the other hand, such as warmth and colour, were thought to have no such independent existence, being, indeed, no more than the resultants due to the action of the primary

qualities on our organs of sense-perception; and here, no doubt, common sense and theory parted company.

You need not fear that I am going to drag you into the controversies with which this theory is historically connected. They have left abiding traces on more than one system of philosophy. They are not yet solved. In the course of them the very possibility of an independent physical universe has seemed to melt away under the solvent powers of critical analysis. But with all this I am not now concerned. I do not propose to ask what proof we have that an external world exists, or how, if it does exist, we are able to obtain cognisance of it. These may be questions very proper to be asked by philosophy; but they are not proper questions to be asked by science. For, logically, they are antecedent to science, and we must reject the sceptical answers to both of them before physical science becomes possible at all. My present purpose requires me to do no more than observe that, be this theory of the primary and secondary qualities of matter good or bad, it is the one on which science has in the main proceeded. It was with matter thus conceived that Newton experimented. To it he applied his laws of motion; of it he predicated universal gravitation. Nor was the case greatly altered when science became as much preoccupied with the movements of molecules as it was with those of planets. For molecules and atoms, whatever else might be said of them, were at least pieces of matter, and, like other pieces of matter, possessed those "primary" qualities supposed to be characteristic of all matter, whether found in large masses or in small.

But the electric theory which we have been considering carries us into a new region altogether. It does not confine itself to accounting for the secondary qualities by the primary, or the behaviour of matter in bulk by the behaviour of matter in atoms; it analyses matter, whether molar or molecular, into something which is not matter at all. The atom is now no more than the relatively vast theatre of operations in which minute monads perform their orderly evolutions; while the monads themselves are not regarded as units of matter, but as units of electricity; so that matter is not merely explained, but is explained away.

Now the point to which I desire to direct attention is not to be sought in the great divergence between matter as thus conceived by the physicist and matter as the ordinary man supposes himself to know it, between matter as it is perceived and matter as it really is, but to the fact that the first of these two quite inconsistent views is wholly based on the second.

This is surely something of a paradox. We claim to found all our scientific opinions on experience; and the experience on which we found our theories of the physical universe is our *sense-perception* of that universe. That is experience; and in this region of belief there is no other. Yet the conclusions which thus profess to be entirely founded upon experience are to all appearance fundamentally opposed to it; our knowledge of reality is based upon illusion, and the very conceptions we use in describing it to others, or in thinking of it ourselves, are abstracted from anthropomorphic fancies, which science forbids us to believe and Nature compels us to employ.

We here touch the fringe of a series of problems with which inductive logic ought to deal, but which that most unsatisfactory branch of philosophy has systematically ignored. This is no fault of men of science. They are occupied in the task of making discoveries, not in that of analysing the fundamental presuppositions which the very possibility of making discoveries implies. Neither is it the fault of transcendental metaphysicians. Their speculations flourish on a different level of thought; their interest in a philosophy of nature is lukewarm; and howsoever the questions in which they are chiefly concerned be answered, it is by no means certain that the answers will leave the humbler difficulties at which I have hinted either nearer to or further from a solution. But though men of science and idealists stand acquitted, the same can hardly be said of empirical philosophers. So far from solving the problem, they seem scarcely to have understood that there was a problem to be solved. Led astray by a misconception to which I have already referred; believing that science was concerned only with (so-called) "phenomena," that it had done all that it could be asked to do if it accounted for the

sequence of our individual sensations, that it was concerned only with the "laws of Nature," and not with the inner character of physical reality; disbelieving, indeed, that any such physical reality does in truth exist;—it has never felt called upon seriously to consider what are the actual methods by which science attains its results, and how those methods are to be justified. If anyone, for example, will take up Mill's logic, with its "sequences and co-existences between phenomena," its "method of difference," its "method of agreement," and the rest; if he will then compare the actual doctrines of science with this version of the mode in which those doctrines have been arrived at,—he will soon be convinced of the exceedingly thin intellectual fare which has been hitherto served out to us under the imposing title of Inductive Theory.

There is an added emphasis given to these reflections by a train of thought which has long interested me, though I acknowledge that it never seems to have interested anyone else. Observe, then, that in order of logic sense-perceptions supply the premisses from which we draw all our knowledge of the physical world. It is they which tell us there is a physical world; it is on their authority that we learn its character. But in order of causation they are effects due (in part) to the constitution of our organs of sense. What we see depends not merely on what there is to be seen, but on our eyes. What we hear depends not merely on what there is to hear, but on our ears. Now, eyes and ears, and all the mechanism of perception, have, as we know, been evolved in us and our brute progenitors by the slow operation of Natural Selection. And what is true of sense-perception is of course also true of the intellectual powers which enable us to erect upon the frail and narrow platform which sense-perception provides, the proud fabric of the sciences.

Now Natural Selection only works through utility. It encourages aptitudes useful to their possessor or his species in the struggle for existence, and, for a similar reason, it is apt to discourage useless aptitudes, however interesting they may be from other points of view, because, being useless, they are probably burdensome.

But it is certain that our powers of sense-perception and of calculation were fully developed ages before they were effectively employed in searching out the secrets of physical reality—for our discoveries in this field are the triumphs but of yesterday. The blind forces of Natural Selection, which so admirably simulate design when they are providing for a present need, possess no power of prevision, and could never, except by accident, have endowed mankind, while in the making, with a physiological or mental outfit adapted to the higher physical investigations. So far as natural science can tell us, every quality of sense or intellect which does not help us to fight, to eat, and to bring up children, is but a by-product of the qualities which do. Our organs of sense-perception were not given us for purposes of research; nor was it to aid us in meting out the heavens or dividing the atom that our powers of calculation and analysis were evolved from the rudimentary instincts of the animal.

It is presumably due to these circumstances that the beliefs of all mankind about the material surroundings in which it dwells are not only imperfect but fundamentally wrong. It may seem singular that down to, say, five years ago, our race has, without exception, lived and died in a world of illusions; and that its illusions, or those with which we are here alone concerned, have not been about things remote or abstract, things transcendental or divine, but about what men see and handle, about those "plain matters of fact" among which common sense daily moves with its most confident step and most self-satisfied smile. Presumably, however, this is either because too direct a vision of physical reality was a hindrance, not a help, in the struggle for existence; because falsehood was more useful than truth; or else because with so imperfect a material as living tissue no better results could be attained. But, if this conclusion be accepted, its consequences extend to other organs of knowledge besides those of perception. Not merely the senses, but the intellect, must be judged by it; and it is hard to see why evolution, which has so lamentably failed to produce trustworthy instruments for obtaining the raw material of experience, should be credited with a larger measure of success in its provision of the physiological

arrangements which condition reason in its endeavours to turn experience to account.

Considerations like these, unless I have compressed them beyond the limits of intelligibility, do undoubtedly suggest a certain inevitable incoherence in any general scheme of thought which is built out of materials provided by natural science alone. Extend the boundaries of knowledge as you may; draw how you will the picture of the universe; reduce its infinite variety to the modes of a single space filling ether; retrace its history to the birth of existing atoms; show how under the pressure of gravitation they became concentrated into nebulae, into suns, and all the host of heaven; how, at least in one small planet, they combined to form organic compounds; how organic compounds became living things; how living things, developing along many different lines, gave birth at last to one superior race; how from this race arose, after many ages, a learned handful, who looked round on the world which thus blindly brought them into being, and judged it, and knew it for what it was:—perform, I say, all this, and, though you may indeed have attained to science, in nowise will you have attained to a self-sufficing system of beliefs. One thing at least will remain, of which this long-drawn sequence of causes and effects gives no satisfying explanation; and that is knowledge itself. Natural science must ever regard knowledge as the product of irrational conditions, for in the last resort it knows no others. It must always regard knowledge as rational, or else science itself disappears. In addition, therefore, to the difficulty of extracting from experience beliefs which experience contradicts, we are confronted with the difficulty of harmonising the pedigree of our beliefs with their title to authority. The more successful we are in explaining their origin, the more doubt we cast on their validity. The more imposing seems the scheme of what we know, the more difficult it is to discover by what ultimate criteria we claim to know it.

Here, however, we touch the frontier beyond which physical science possesses no jurisdiction. If the obscure and difficult region which lies beyond is to be surveyed and made accessible, philosophy, not science, must undertake the task. It is no business of this Society. We meet here to promote the cause of knowledge in one of its great divisions; we shall not help it by confusing the limits which usefully separate one division from another. It may perhaps be thought that I have disregarded my own precept—that I have willfully overstepped the ample bounds within which the searchers into Nature carry on their labours. If it be so, I can only beg your forgiveness. My first desire has been to rouse in those who, like myself, are no specialists in physics, the same absorbing interest which I feel in what is surely the most far-reaching speculation about the physical universe which has ever claimed experimental support; and if in so doing I have been tempted to hint my own personal opinion that as natural science grows it leans more, not less, upon an idealistic interpretation of the universe, even those who least agree may perhaps be prepared to pardon.

SECTION A.

MATHEMATICS AND PHYSICS.

OPENING ADDRESS BY PROF. HORACE LAMB, LL.D., D.Sc.,
F.R.S., PRESIDENT OF THE SECTION.

THE losses sustained by mathematical science in the past twelvemonth have perhaps not been so numerous as in some years, but they include at least one name of world-wide import. Those of us who were students of Mathematics thirty or forty years ago will recall the delight which we felt in reading the geometrical treatises of George Salmon, and the brilliant contrast which they exhibited with most of the current text-books of that time. It was from him that many of us first learned that a great mathematical theory does not consist of a series of detached propositions carefully labelled and arranged like specimens on the shelves of a museum, but that it forms an organic whole, instinct with life, and with unlimited possibilities of future development. As systematic expositions of the actual state of the science, in which enthusiasm for what is new is tempered by a due respect for what is old, and in which new and old are brought into harmonious relation with each other, these treatises stand almost unrivalled.

Whether in the originals, or in the guise of translations, they are accounted as classics in every university of the world. So far as British universities are concerned, they have formed the starting-point of a whole series of works conceived in a similar spirit, though naturally not always crowned by the same success. The necessity for this kind of work grows, indeed, continually; the modern fragmentary fashion of original publication and the numerous channels through which it takes place make it difficult for anyone to become initiated into a new scientific theory unless he takes it up at the very beginning and follows it diligently throughout its course, backwards and forwards, over rough ground and smooth. The classical style of memoir, after the manner of Lagrange, or Poisson, or Gauss, complete in itself and deliberately composed like a work of art, is continually becoming rarer. It is, therefore, more and more essential that from time to time some one should come forward to sort out and arrange the accumulated material, rejecting what has proved unimportant, and welding the rest into a connected system. There is perhaps a tendency to assume that such work is of secondary importance, and can be safely left to subordinate hands. But in reality it makes severe demands on even the highest powers; and when these have been available the result has often done more for the progress of science than the composition of a dozen monographs on isolated points. For proof one need only point to the treatises of Salmon himself, or recall (in another field) the debt which we owe to such books as the "Treatise on Natural Philosophy" and the "Theory of Sound," whose authors are happily represented amongst us.

A modest but most valuable worker has passed away in the person of Prof. Allman. His treatise on the history of Greek Geometry, full of learning and sound mathematical perception, is written with great simplicity, and an entire absence of pedantry or dogmatism. It ranks, I believe, with the best that has been done in the subject. It is to be regretted that, as an historian, he leaves so few successors among British mathematicians. We have amongst us, as a result of our system of university education, many men of trained mathematical faculty and of a scholarly turn of mind, with much of the necessary linguistic equipment, who feel, however, no special vocation for the details of recent mathematical research. Might not some of this ability be turned to a field, by no means exhausted, where the severity of mathematical truth is tempered by the human interest attaching to the lives, the vicissitudes, and even the passions and the strife of its devotees, who through many errors and perplexities have contrived to keep alive and trim the sacred flame, and to hand it on burning ever clearer and brighter?

In another province we have to record the loss of Dr. Isaac Roberts, a distinguished example of the class of non-professional investigators who have left so deep a mark on British science and on Astronomy in particular. Few of us can be unaware of his long and enthusiastic devotion to celestial photography, of the beauty and delicacy of the results which he achieved, or of the wealth of unsuspected detail which they brought to light.

Finally, we have to lament the death, within the last few days, of Prof. Everett, whose name will always be associated with one of the most successful tasks which the British Association has taken in hand, viz., the promotion of a uniform system of dynamical and electrical units. He acted as Reporter to the Committee entrusted with the question, and by his handbook on "Units and Physical Constants," he has done more, perhaps, than anyone else to popularise and establish its recommendations. He was well known to most of us as a bright and genial presence at these meetings, and contributed numerous interesting papers on optical and other subjects. He was happy in retaining his scientific faculties undimmed to the last, and was engaged at the time of his death on some problems of a mathematical kind, on point-assemblages, suggested by a study of the recent speculations of Prof. Osborne Reynolds.

Of the various subjects which fall within the scope of this Section there is no difficulty in naming that which at the present time excites the widest interest. The phenomena of Radio-activity, Ionisation of Gases, and so on, are not only startling and sensational in themselves, they

have suggested most wonderful and far-reaching speculations, and, whatever be the future of these particular theories, they are bound in any case deeply to influence our views on fundamental points of chemistry and physics. No reference to this subject would, I think, be satisfactory without a word of homage to the unsurpassed patience and skill in the devising of new experimental methods to meet new and subtle conditions which it has evoked. It will be felt as a matter of legitimate pride by many present that the University of Cambridge has been so conspicuously associated with this work. It would therefore have been natural and appropriate that this Chair should have been occupied, this year above others, by one who could have given us a survey of the facts as they at present stand, and of their bearing, so far as can be discerned, on other and older branches of physics. Whether from the experimental or from the more theoretical and philosophical standpoint, there would have been no difficulty in finding exponents of unrivalled authority. But it has been otherwise ordered, and you and I must make the best of it. If the subject cannot be further dealt with for the moment, we have the satisfaction of knowing that it will in due course engage the attention of the Section, and that we may look forward to interesting and stimulating discussions, in which we trust the many distinguished foreign physicists who honour us by their presence will take an active part.

It is, I believe, not an unknown thing for your President to look up the records of previous meetings in search of inspiration, and possibly of an example. I have myself not had to look very far, for I found that when the British Association last met in Cambridge, in the year 1862, this Section was presided over by Stokes, and, moreover, that the Address which he gave was probably the shortest ever made on such an occasion, for it occupies only half a page of the report, and took, I should say, some three or four minutes to deliver. It would be to the advantage of the business of the meeting, and to my own great relief, if I had the courage to follow so attractive a precedent; but I fear that the tradition which has since established itself is too strong for me to break without presumption. I will turn, therefore, in the first instance, to a theme which, I think, naturally presents itself—viz., a consideration of the place occupied by Stokes in the development of Mathematical Physics. It is not proposed to attempt an examination or appreciation of his own individual achievements; this has lately been done by more than one hand, and in the most authoritative manner. But it is part of the greatness of the man that his work can be reviewed from more than one standpoint. What I specially wish to direct attention to on this occasion is the historical or evolutionary relation in which he stands to predecessors and followers in the above field.

The early years of Stokes's life were the closing years of a mighty generation of mathematicians and mathematical physicists. When he came to manhood, Lagrange, Laplace, Poisson, Fourier, Fresnel, Ampère, had but recently passed away. Cauchy alone of this race of giants was still alive and productive. It is upon these men that we must look as the immediate intellectual ancestors of Stokes, for, although Gauss and F. Neumann were alive and flourishing, the interaction of German and English science was at that time not very great. It is noteworthy, however, that the development of the modern German school of mathematical physics, represented by Helmholtz and Kirchhoff, in linear succession to Neumann, ran in many respects closely parallel to the work of Stokes and his followers.

When the foundations of Analytical Dynamics had been laid by Euler and d'Alembert, the first important application was naturally to the problems of Gravitational Astronomy; this formed, of course, the chief work of Laplace, Lagrange, and others. Afterwards came the theoretical study of Elasticity, Conduction of Heat, Statical Electricity, and Magnetism. The investigations in Elasticity were undertaken mainly in relation to Physical Optics, with the hope of finding a material medium capable of conveying transverse vibrations, and of accounting also for the various phenomena of reflection, refraction, and double refraction. It has often been pointed out, as characteristic of the French school referred to, that their physical speculations were largely influenced by ideas transferred from

Astronomy; as, for instance, in the conception of a solid body as made up of discrete particles acting on one another at a distance with forces in the lines joining them, which formed the basis of most of their work on Elasticity and Optics. The difficulty of carrying out these ideas in a logical manner was enormous, and the strict course of mathematical deduction had to be replaced by more or less precarious assumptions. The detailed study of the geometry of a continuous deformable medium which was instituted by Cauchy was a first step towards liberating the theory from arbitrary and unnecessary hypothesis; but it was reserved for Green, the immediate predecessor of Stokes among English mathematicians, to carry out this process completely and independently, with the help of Lagrange's general dynamical methods, which here found their first application to questions of physics outside the ordinary Dynamics of rigid bodies and fluids. The modern school of English physicists, since the time of Green and Stokes, have consistently endeavoured to make out, in any given class of phenomena, how much can be recognised as a manifestation of general dynamical principles, independent of the particular mechanism which may be at work. One of the most striking examples of this was the identification by Maxwell of the laws of Electromagnetism with the dynamical equations of Lagrange. It would, however, be going too far to claim this tendency as the exclusive characteristic of English physicists; for example, the elastic investigations of Green and Stokes have their parallel in the independent though later work of Kirchhoff; and the beautiful theory of dynamical systems with latent motion which we owe to Lord Kelvin stands in a very similar relation to the work of Helmholtz and Hertz.

But perhaps the most important and characteristic feature in the mathematical work of the later school is its increasing relation to and association with experiment. In the days when the chief applications of Mathematics were to the problems of Gravitational Astronomy, the mathematician might well take his materials at second hand; and in some respects the division of labour was, and still may be, of advantage. The same thing holds, in a measure, of the problems of ordinary Dynamics, where some practical knowledge of the subject-matter is within the reach of everyone. But when we pass to the more recondite phenomena of Physical Optics, Acoustics, and Electricity, it hardly needs the demonstrations which have involuntarily been given to show that the theoretical treatment must tend to degenerate into the pursuit of mere academic subtleties unless it is constantly vivified by direct contact with reality. Stokes, at all events, with little guidance or encouragement from his immediate environment, made himself from the first practically acquainted with the subjects he treated. Generations of Cambridge students recall the enthusiasm which characterised his experimental demonstrations in Optics. These appealed to us all; but some of us, I am afraid, under the influence of the academic ideas of the time, thought it a little unnecessary to show practically that the height of the lecture-room could be measured by the barometer, or to verify the calculated period of oscillation of water in a tank by actually timing the waves with the help of the image of a candle-flame reflected at the surface.

The practical character of the mathematical work of Stokes and his followers is shown especially in the constant effort to reduce the solution of a physical problem to a quantitative form. A conspicuous instance is furnished by the labour and skill which he devoted, from this point of view, to the theory of the Bessel's Function, which presents itself so frequently in important questions of Optics, Electricity, and Acoustics, but is so refractory to ordinary methods of treatment. It is now generally accepted that an analytical solution of a physical question, however elegant it may be made to appear by means of a judicious notation, is not complete so long as the results are given merely in terms of functions defined by infinite series or definite integrals, and cannot be exhibited in a numerical or graphical form. This view did not originate, of course, with Stokes; it is clearly indicated, for instance, in the works of Fourier and Poincaré, but no previous writer had, I think, acted upon it so consistently and thoroughly.

We have had so many striking examples of the fruitfulness of the combination of great mathematical and experi-

mental powers that the question may well be raised, whether there is any longer a reason for maintaining in our minds a distinction between mathematical and experimental physics, or at all events whether these should be looked upon as separate provinces which may conveniently be assigned to different sets of labourers. It may be held that the highest physical research will demand in the future the possession of both kinds of faculty. We must be careful, however, how we erect barriers which would exclude a Lagrange on the one side or a Faraday on the other. There are many mansions in the palace of physical science, and work for various types of mind. A zealous, or over-zealous, mathematician might indeed make out something of a case if he were to contend that, after all, the greatest work of such men as Stokes, Kirchhoff, and Maxwell was mathematical rather than experimental in its complexion. An argument which asks us to leave out of account such things as the investigation of Fluorescence, the discovery of Spectrum Analysis, and the measurement of the Viscosity of Gases, may seem audacious; but a survey of the collected works of these writers will show how much, of the very highest quality and import, would remain. However this may be, the essential point, which cannot, I think, be contested, is this, that if these men had been condemned and restricted to a mere book knowledge of the subjects which they have treated with such marvellous analytical ability, the very soul of their work would have been taken away. I have ventured to dwell upon this point because, although I am myself disposed to plead for the continued recognition of mathematical physics as a fairly separate field, I feel strongly that the traditional kind of education given to our professed mathematical students does not tend to its most effectual cultivation. This education is apt to be one-sided, and too much divorced from the study of tangible things. Even the student whose tastes lie mainly in the direction of pure mathematics would profit, I think, by a wider scientific training. A long list of instances might be given to show that the most fruitful ideas in pure mathematics have been suggested by the study of physical problems. In the words of Fourier, who did so much to fulfil his own saying, "*L'étude approfondie de la nature est la source la plus féconde des découvertes mathématiques. Non seulement cette étude, en offrant aux recherches un but déterminé, a l'avantage d'exclure les questions vagues et les calculs sans issue; elle est encore un moyen assuré de former l'analyse elle-même, et d'en découvrir les éléments qu'il nous importe le plus de connaître, et que cette science doit toujours conserver: ces éléments fondamentaux sont ceux qui se reproduisent dans tous les effets naturels.*"

Another characteristic of the applied mathematics of the past century is that it was, on the whole, the age of linear equations. The analytical armoury fashioned by Lagrange, Poisson, Fourier, and others, though subject, of course, to continual improvement and development, has served the turn of a long line of successors. The predominance of linear equations, in most of the physical subjects referred to, rests on the fact that the changes are treated as infinitely small. The electric theory of light forms at present an exception; but even here the linear character of the fundamental electrical relations is itself remarkable, and possibly significant. The theory of small oscillations, in particular, runs as a thread through a great part of the literature of the period in question. It has suggested many important analytical results, and it still gives the best and simplest intuitive foundation for a whole class of theorems which are otherwise hard to comprehend in their various relations, such as Fourier's theorem, Laplace's expansion, Bessel's functions, and the like. Moreover, the interest of the subject, whether mathematical or physical, is not yet exhausted; many important problems in Optics and Acoustics, for example, still await solution. The general theory has in comparatively recent times received an unexpected extension (to the case of "latent motions") at the hands of Lord Kelvin; and Lord Rayleigh, by his continual additions to it, shows that, in his view, it is still incomplete.

When the restriction to infinitely small motions is abandoned, the problems become of course much more arduous. The whole theory, for instance, of the normal modes of vibration which is so important in Acoustics, and

even in Music, disappears. The researches hitherto made in this direction have, moreover, encountered difficulties of a less patent character. It is conceivable that the modern analytical methods which have been developed in Astronomy may have an application to these questions. It would appear that there is an opening here for the mathematician; at all events, the numerical or graphical solution of any one of the various problems that could be suggested would be of the highest interest. One problem of the kind is already classical—the theory of steep water-waves discussed by Stokes; but even here the point of view has perhaps been rather artificially restricted. The question proposed by him, the determination of the possible form of waves of permanent type, like the problem of periodic orbits in Astronomy, is very interesting mathematically, and forms a natural starting-point for investigation; but it does not exhaust what is most important for us to know in the matter. Observation may suggest the existence of such waves as a fact; but no reason has been given, so far as I know, why free water-waves should tend to assume a form consistent with permanence, or be influenced in their progress by considerations of geometrical simplicity.

I have tried to indicate the kind of continuity of subject-matter, method, and spirit which runs through the work of the whole school of mathematical physicists of which Stokes may be taken as the representative. It is no less interesting, I think, to examine the points of contrast with more recent tendencies. These relate not so much to subject-matter and method as to the general mental attitude towards the problems of Nature. Mathematical and physical science have become markedly introspective. The investigators of the classical school, as it may perhaps be styled, were animated by a simple and vigorous faith; they sought as a matter of course for a mechanical explanation of phenomena, and had no misgivings as to the trustiness of the analytical weapons which they wielded. But now the physicist and the mathematician alike are in trouble about their souls. We have discussions on the principles of mechanics, on the foundations of geometry, on the logic of the most rudimentary arithmetical processes, as well as of the more artificial operations of the Calculus. These discussions are legitimate and inevitable, and have led to some results which are now widely accepted. Although they were carried on to a great extent independently, the questions involved will, I think, be found to be ultimately very closely connected. Their common nexus is, perhaps, to be traced in the physiological ideas of which Helmholtz was the most conspicuous exponent. To many minds such discussions are repellent, in that they seem to venture on the uncertain ground of philosophy. But, as a matter of fact, the current views on these subjects have been arrived at by men who have gone to work in their own way, often in entire ignorance of what philosophers have thought on such subjects. It may be maintained, indeed, that the mathematician or the physicist, as such, has no special concern with philosophy, any more than the engineer or the geographer. Nor, although this is a matter for their own judgment, would it appear that philosophers have very much to gain by a special study of the methods of mathematical or physical reasoning, since the problems with which they are chiefly concerned are presented to them in a much less artificial form in the circumstances of ordinary life. As regards the present topic I would put the matter in this way, that between Mathematics and Physics on the one hand and Philosophy on the other there lies an undefined borderland, and that the mathematician has been engaged in setting things in order, as he is entitled to do, on his own side of the boundary.

Adopting this point of view, it would be of interest to trace in detail the relationships of the three currents of speculation which have been referred to. At one time, indeed, I was tempted to take this as the subject of my Address; but, although I still think the enterprise a possible one, I have been forced to recognise that it demands a better equipment than I can pretend to. I can only venture to put before you some of my tangled thoughts on the matter, trusting that some future occupant of this Chair may be induced to take up the question and treat it in a more illuminating manner.

If we look back for a moment to the views currently entertained not so very long ago by mathematicians and

physicists, we shall find, I think, that the prevalent conception of the world was that it was constructed on some sort of absolute geometrical plan, and that the changes in it proceeded according to precise laws; that, although the principles of mechanics might be imperfectly stated in our text-books, at all events such principles existed, and were ascertainable, and, when properly formulated, would possess the definiteness and precision which were held to characterise, say, the postulates of Euclid. Some writers have maintained, indeed, that the principles in question were finally laid down by Newton, and have occasionally used language which suggests that any fuller understanding of them was a mere matter of interpretation of the text. But, as Hertz has remarked, most of the great writers on Dynamics betray, involuntarily, a certain *malaise* when explaining the principles, and hurry over this part of their task as quickly as is consistent with dignity. They are not really at their ease until, having established their equations somehow, they can proceed to build securely on these. This has led some people to the view that the laws of Nature are merely a system of differential equations; it may be remarked in passing that this is very much the position in which we actually stand in some of the more recent theories of Electricity. As regards Dynamics, when once the critical movement had set in, it was easy to show that one presentation after another was logically defective and confused; and no satisfactory standpoint was reached until it was recognised that in the classical Dynamics we do not deal immediately with real bodies at all, but with certain conventional and highly idealised representations of them, which we combine according to arbitrary rules, in the hope that if these rules be judiciously framed the varying combinations will image to us what is of most interest in some of the simpler and more important phenomena. The changed point of view is often associated with the publication of Kirchhoff's lectures on Mechanics in 1876, where it is laid down in the opening sentence that the problem of Mechanics is to describe the motions which occur in Nature completely and in the simplest manner. This statement must not be taken too literally; at all events, a fuller, and I think a clearer, account of the province and the method of Abstract Dynamics is given in a review of the second edition of Thomson and Tait, which was one of the last things penned by Maxwell, in 1879 (NATURE, vol. xx. p. 213; *Scientific Papers*, vol. ii. p. 776). A "complete" description of even the simplest natural phenomenon is an obvious impossibility; and, were it possible, it would be uninteresting as well as useless, for it would take an incalculable time to peruse. Some process of selection and idealisation is inevitable if we are to gain any intelligent comprehension of events. Thus, in Astronomy we replace a planet by a so-called material particle—i.e., a mathematical point associated with a suitable numerical coefficient. All the properties of the body are here ignored except those of position and mass, in which alone we are at the moment interested. The whole course of physical science and the language in which its results are expressed have been largely determined by the fact that the ideal images of Geometry were already at hand at its service. The ideal representations have the advantage that, unlike the real objects, definite and accurate statements can be made about them. Thus two lines in a geometrical figure can be pronounced to be equal or unequal, and the statement is in either case absolute. It is no doubt hard to divest oneself entirely of the notion conveyed in the Greek phrase *ἀεὶ ὁ θεὸς γεωμετρεῖ*, that definite geometrical magnitudes and relations are at the back of phenomena. It is recognised indeed that all our measurements are necessarily to some degree uncertain, but this is usually attributed to our own limitations and those of our instruments rather than to the ultimate vagueness of the entity which it is sought to measure. Everyone will grant, however, that the distance between two clouds, for instance, is not a definable magnitude; and the distance of the earth from the sun, and even the length of a wave of light, are in precisely the same case. The notion in question is a convenient fiction, and is a striking testimony to the ascendancy which Greek Mathematics have gained over our minds, but I do not think that more can be said for it. It is, at any rate, not verified by the experience of those who actually undertake physical measurements. The more refined the means employed, the

more vague and elusive does the supposed magnitude become; the judgment flickers and wavers, until at last in a sort of despair *some* result is put down, not in the belief that it is exact, but with the feeling that it is the best we can make of the matter. A practical measurement is in fact a classification; we assign a magnitude to a certain category, which may be narrowly limited, but which has in any case a certain breadth.

By a frank process of idealisation a logical system of Abstract Dynamics can doubtless be built up, on the lines sketched by Maxwell in the passage referred to. Such difficulties as remain are handed over to Geometry. But we cannot stop in this position; we are constrained to examine the nature and the origin of the conceptions of Geometry itself. By many of us, I imagine, the first suggestion that these conceptions are to be traced to an empirical source was received with something of indignation and scorn; it was an outrage on the science which we had been led to look upon as divine. Most of us have, however, been forced at length to acquiesce in the view that Geometry, like Mechanics, is an applied science; that it gives us merely an ingenious and convenient symbolic representation of the relations of actual bodies; and that, whatever may be the *a priori* forms of intuition, the science as we have it could never have been developed except for the accident (if I may so term it) that we live in a world in which rigid or approximately rigid bodies are conspicuous objects. On this view the most refined geometrical demonstration can be resolved into a series of imagined experiments performed with such bodies, or rather with their conventional representations.

It is to be lamented that one of the most interesting chapters in the history of science is a blank; I mean that which would have unfolded the rise and growth of our system of ideal Geometry. The finished edifice is before us, but the record of the efforts by which the various stones were fitted into their places is hopelessly lost. The few fragments of professed history which we possess were edited long after the achievement.

It is commonly reckoned that the first rude beginnings of Geometry date from the Egyptians. I am inclined to think that in one sense the matter is to be placed much further back, and that the dawn of geometric ideas is to be traced among the prehistoric races who carved rough but thoroughly artistic outlines of animals on their weapons. I do not know whether the matter has attracted serious speculation, but I have myself been led to wonder how men first arrived at the notion of an outline drawing. The primitive sketches referred to immediately convey to the experienced mind the idea of a reindeer or the like; but in reality the representation is purely conventional, and is expressed in a language which has to be learned. For nothing could be more unlike the actual reindeer than the few scratches drawn on the surface of a bone; and it is of course familiar to ourselves that it is only after a time, and by an insensible process of education, that very young children come to understand the meaning of an outline. Whoever he was, the man who first projected the world into two dimensions, and proceeded to fence off that part of it which was reindeer from that which was not, was certainly under the influence of a geometrical idea, and had his feet in the path which was to culminate in the refined idealisations of the Greeks. As to the manner in which these latter were developed, the only indication of tradition is that some propositions were arrived at first in a more empirical or intuitional, and afterwards in a more intellectual way. So long as points had size, lines had breadth, and surfaces thickness, there could be no question of exact relations between the various elements of a figure, any more than is the case with the realities which they represent. But the Greek mind loved definiteness, and discovered that if we agree to speak of lines as if they had no breadth, and so on, exact statements became possible. If any one scientific invention can claim pre-eminence over all others, I should be inclined myself to erect a monument to the inventor of the mathematical point, as the supreme type of that process of abstraction which has been a necessary condition of scientific work from the very beginning.

It is possible, however, to uphold the importance of the part which Abstract Geometry has played, and must still play, in the evolution of scientific conceptions, without committing ourselves to a defence, on all points, of the

traditional presentment. The consistency and completeness of the usual system of definitions, axioms, and postulates have often been questioned; and quite recently a more thorough-going analysis of the logical elements of the subject than has ever before been attempted has been made by Hilbert. The matter is a subtle one, and a general agreement on such points is as yet hardly possible. The basis for such an agreement may perhaps ultimately be found in a more explicit recognition of the empirical source of the fundamental conceptions. This would tend, at all events, to mitigate the rigour of the demands which are sometimes made for logical perfection.

Even more important in some respects are the questions which have arisen in connection with the applications of Geometry to purposes of graphical representation. It is not necessary to dwell on the great assistance which this method has rendered in such subjects as Physics and Engineering. The pure mathematician, for his part, will freely testify to the influence which it has exercised in the development of most branches of Analysis; for example, we owe to it all the leading ideas of the Calculus. Modern analysts have discovered, however, that Geometry may be a snare as well as a guide. In the mere act of drawing a curve to represent an analytical function we make unconsciously a host of assumptions which are difficult not merely to prove, but even to formulate precisely. It is now sought to establish the whole fabric of mathematical analysis on a strictly arithmetical basis. To those who were trained in an earlier school, the results so far are in appearance somewhat forbidding. If the shade of one of the great analysts of a century ago could revisit the glimpses of the moon, his feelings would, I think, be akin to those of the traveller to some mediæval town, who finds the buildings he came to see obscured by scaffolding, and is told that the ancient monuments are all in process of repair. It is to be hoped that a good deal of this obstruction is only temporary, that most of the scaffolding will eventually be cleared away, and that the edifices when they reappear will not be entirely transformed, but will still retain something of their historic outlines. It would be contrary to the spirit of this Address to undervalue in any way the critical examination and revision of principles; we must acknowledge that it tends ultimately to simplification, to the clearing up of issues, and the reconciliation of apparent contradictions. But it would be a misfortune if this process were to absorb too large a share of the attention of mathematicians, or were allowed to set too high a standard of logical completeness. In this particular matter of the "arithmetisation of Mathematics" there is, I think, a danger in these respects. As regards the latter point, a traveller who refuses to pass over a bridge until he has personally tested the soundness of every part of it is not likely to go very far; something must be risked, even in Mathematics. It is notorious that even in this realm of "exact" thought discovery has often been in advance of strict logic, as in the theory of imaginaries, for example, and in the whole province of analysis of which Fourier's theorem is the type. And it might even be claimed that the services which Geometry has rendered to other sciences have been almost as great in virtue of the questions which it implicitly begs as of those which it resolves.

I would venture, with some trepidation, to go one step further. Mathematicians love to build on as definite a foundation as possible, and from this point of view the notion of the integral number, on which (we are told) the Mathematics of the future are to be based, is very attractive. But, as an instrument for the study of Nature, is it really more fundamental than the geometrical notions which it is to supersede? The accounts of primitive peoples would seem to show that, in the generality which is a necessary condition for this purpose, it is in no less degree artificial and acquired. Moreover, does not the act of enumeration, as applied to actual things, involve the very same process of selection and idealisation which we have already met with in other cases? As an illustration, suppose we were to try to count the number of drops of water in a cloud. I am not thinking of the mere practical difficulties of enumeration, or even of the more pertinent fact that it is hard to say where the cloud begins or ends. Waiving these points, it is obvious that there must be transitional stages between a more or less dense group of molecules and a drop, and in the case of some of these

aggregates it would only be by an arbitrary exercise of judgment that they would be assigned to one category rather than to the other. In whatever form we meet with it, the very notion of counting involves the highly artificial conception of a number of objects which for some purposes are treated as absolutely alike, whilst yet they can be distinguished.

The net result of the preceding survey is that the systems of Geometry, of Mechanics, and even of Arithmetic, on which we base our study of Nature, are all contrivances of the same general kind: they consist of series of abstractions and conventions devised to represent, or rather to symbolise, what is most interesting and most accessible to us in the world of phenomena. And the progress of science consists in a great measure in the improvement, the development, and the simplification of these artificial conceptions, so that their scope may be wider and the representation more complete. The best in this kind are but shadows, but we may continually do something to amend them.

As compared with the older view, the function of physical science is seen to be much more modest than was at one time supposed. We no longer hope by levers and screws to pluck out the heart of the mystery of the universe. But there are compensations. The conception of the physical world as a mechanism, constructed on a rigid mathematical plan, whose most intimate details might possibly some day be guessed, was, I think, somewhat depressing. We have been led to recognise that the formal and mathematical element is of our own introduction; that it is merely the apparatus by which we map out our knowledge, and has no more objective reality than the circles of latitude and longitude on the sun. A distinguished writer not very long ago speculated on the possibility of the scientific mine being worked out within no distant period. Recent discoveries seem to have put back this possibility indefinitely; and the tendency of modern speculation as to the nature of scientific knowledge should be to banish it altogether. The world remains a more wonderful place than ever; we may be sure that it abounds in riches not yet dreamed of; and although we cannot hope ever to explore its innermost recesses, we may be confident that it will supply tasks in abundance for the scientific mind for ages to come.

One significant result of the modern tendency is that we no longer with the same obstinacy demand a mechanical explanation of the phenomena of Light and Electricity, especially since it has been made clear that if one mechanical explanation is possible, there will be an infinity of others. Some minds, indeed, revelling in their new-found freedom, have attempted to disestablish ordinary or "vulgar" matter altogether. I may refer to a certain treatise which, by some accident, does not bear its proper title of "Æther and no Matter," and to the elaborate investigations of Prof. Osborne Reynolds, which present the same peculiarity, although the basis is different. Speculations of this nature have, however, been so recently and (if I may say it) so brilliantly dealt with by Prof. Poynting before this Section that there is little excuse for dwelling further on them now. I will only advert to the question whether, as some suggest, physical science should definitely abandon the attempt to construct mechanical theories in the older sense. The question would appear to be very similar to this, whether we should abandon the use of graphical methods in analysis. In either case we run the risk of introducing extraneous elements, possibly of a misleading character; but the gain in vividness of perception and in suggestiveness is so great that we are not likely altogether to forego it, by excess of prudence, in one case more than in the other.

We have travelled some distance from Stokes and the mathematical physics of half a century ago. May I add a few observations which might perhaps have claimed his sympathy? They are in substance anything but new, although I do not find them easy to express. We have most of us frankly adopted the empirical attitude in physical science; it has justified itself abundantly in the past, and has more and more forced itself upon us. We have given up the notion of causation, except as a convenient phrase; what were once called laws of Nature are now simply rules by which we can tell more or less

accurately what will be the consequences of a given state of things. We cannot help asking, How is it that such rules are possible? A rule is invented in the first instance to sum up in a compact form a number of past experiences; but we apply it with little hesitation, and generally with success, to the prediction of new and sometimes strange ones. Thus the law of gravitation indicates the existence of Neptune; and Fresnel's wave-surface gives us the quite unsuspected phenomenon of double refraction. Why does Nature make a point of honouring our cheques in this manner, or, to put the matter in a more dignified form, how comes it that, in the words of Schiller,¹

"Mit dem Genius steht die Natur im ewigen Bunde,
Was der eine verspricht, leistet die andre gewiss?"

The question is as old as science, and the modern tendencies with which we have been occupied have only added point to it. It is plain that physical science has no answer; its policy, indeed, has been to retreat from a territory which it could not securely occupy. We are told in some quarters that it is vain to look for an answer anywhere. But the mind of man is not wholly given over to physical science, and will not be content for ever to leave the question alone. It will persist in its obstinate questionings, and, however hopeless the attempt to unravel the mystery may be deemed, physical science, powerless to assist, has no right to condemn it.

I would like, in conclusion, to read to you a characteristic passage from that Address of Stokes in 1862 which has formed the starting-point of this discourse:—

"In this Section, more, perhaps, than in any other, we have frequently to deal with subjects of a very abstract character, which in many cases can be mastered only by patient study, at leisure, of what has been written. The question may not unnaturally be asked, If investigations of this kind can best be followed by quiet study in one's own room, what is the use of bringing them forward in a Sectional meeting at all? I believe that good may be done by public mention, in a meeting like the present, of even somewhat abstract investigations; but whether good is thus done, or the audience merely wearied to no purpose, depends upon the judiciousness of the person by whom the investigation is brought forward."

It might be urged that these remarks are as pertinent now as they were forty years ago, but I will leave them on their own weighty authority. I will not myself attempt to emphasise them, lest some of my hearers should be tempted to retort that the warning might well be borne in mind, not only in the ordinary proceedings of the Section, but in the composition of a Presidential Address!

SECTION B.

CHEMISTRY.

OPENING ADDRESS BY PROF. SYDNEY YOUNG, D.Sc., F.R.S.,
PRESIDENT OF THE SECTION.

THE researches of Hermann Kopp on the molecular volumes and boiling-points of chemical compounds extended over half a century, beginning with his inaugural dissertation on the densities of oxides in 1838, and concluding in 1889 with a review of the whole of the work done on the subject. In his second paper Kopp considered the molecular volumes of solid compounds, and arrived at the conclusion that truly isomorphous substances have the same atomic or molecular volume, but that in other cases the volumes are usually different. Schröder also made the same observation at about the same time.

Now, isomorphous substances have analogous chemical formulæ, and are usually of similar chemical character, and it is interesting to notice that at this early date the fact was recognised that close chemical relationship is associated with similarity in physical properties.

For about the first six years Kopp was engaged in the consideration of the results obtained by other observers, and from these results he deduced the most important of his generalisations.

As regards boiling-points, Kopp, in 1842, concluded that a constant difference in chemical composition is accompanied by a constant difference in boiling-point, and he adopted

the value 18° as the rise due to the replacement of the methyl by the ethyl group in organic compounds, although the observed differences varied between 11°·0 and 24°·8. Two years later he found in sixteen comparisons differences varying from 8° to 33°; but he doubted the correctness of the extreme values, and took 19° as the true value; he further suggested that this is the constant difference for an addition of CH₂ in any homologous series, and he pointed out that the observed difference was most regular in the case of the fatty acids.

Kopp was also of opinion that isomeric compounds with the same composition and the same vapour density have the same boiling-point.

The paucity of experimental data and the wide discrepancies between the results obtained by different observers induced Kopp to undertake the determination of the boiling-points of various compounds, and, later, their molecular volumes at a series of temperatures, and it is interesting to note the comparative crudeness of his first attempts and the increasing attention which he paid to the purification of his compounds and to the elimination of thermometric and other errors. He first examined three pairs of esters in order to find whether isomeric compounds have really the same boiling-points. But he employed only calcium chloride as a dehydrating agent, and this would remove neither water nor the alcohol completely; he was much troubled by the "bumping" of the liquids, and the temperatures he actually observed—with the thermometer bulb in the liquid—fluctuated considerably, and he could only, in most cases, take the lowest temperature observed as the most probable boiling-point. By so doing, and by making a fairly liberal allowance for residual errors, Kopp arrived at the erroneous conclusion that the boiling-points of isomers were the same in the three cases examined, and therefore, probably, in all cases.

The boiling-point of methyl alcohol was of great interest to Kopp, because, taking that of ethyl alcohol—about which there was general agreement—as correct, it should, according to his law, be 78°–19°=59°, while the temperatures actually observed varied from 60° to 66°. Kopp prepared a specimen of methyl alcohol, and found that it boiled at about 65°; but he had more faith in his law than in his experimental result, and he concluded that the methods of determining boiling-points were not sufficiently accurate to give results correct to within even 1° or 2°.

In 1854 he discussed the corrections which should be applied to thermometer readings, giving a table of corrections for the unheated column of mercury, and adopting the value 27 mm. per degree as the value of dp/dt for all substances, in order to reduce the observed boiling-point to that at normal pressure. He pointed out, also, that the height of the barometer should be reduced to 0° C. Taking advantage of Delff's improved method of preparing and purifying methyl alcohol, Kopp made a fresh specimen from methyl oxalate and dried it with lime; but while Delff observed the boiling-point to be 60°, Kopp obtained the value 65°·2–65°·8. He was still, however, inclined to think that, owing to bumping, the observed boiling-point was too high and that the true temperature should be about 60°.

Meanwhile, in 1847 Kopp had examined sixteen liquids, including water, two alcohols, three fatty acids, and seven esters, and in 1854, as a result of his further determinations, he was able to compare the boiling-points—and also the molecular volumes—of a large number of substances, most of which were either alcohols, acids, or esters, and he at first adhered to his previous value of 19° for the rise of boiling-point due to the addition of CH₂. Later in the same year, however, taking a wider survey and including hydrocarbons and their halogen derivatives, ethers, sulphides, and other compounds, he was obliged to admit that the difference is in some cases higher, in others lower, than 19°, but he still regarded these cases merely as exceptions to the law. In 1867 Kopp admitted that isomeric aromatic hydrocarbons have not always the same boiling-point, and that the difference for an addition of CH₂ was not always 19°; but he still believed that the difference for CH₂ was constant in any really homologous series—for example, 20°·5 for homologues of toluene, 18°·5 for those of xylene, and 16°·5 for those of trimethylbenzene. He also recognised the fact that isomeric alcohols have widely different boiling-points.

Kopp published no later papers on the boiling-points of

¹ Applied by Herschel to the discovery of Neptune.

organic compounds, although he dealt fully with the question of molecular volumes in his final communication in 1889.

As a pioneer, Kopp had very great difficulties to contend against when he began his researches; data were scanty and far from accurate, and the substances which could be most easily obtained and, it was thought, readily purified were, unfortunately, those which were the least likely to lead to normal generalisations. Water, the alcohols, and the organic acids all contain a hydroxyl group, and we now know that the physical properties of these substances are abnormal in nearly all respects, owing, probably, to the fact that their molecules tend to associate together; moreover, the esters, which are formed by the interaction of acids and alcohols, do not behave quite normally, and there is probably molecular association, though to a much smaller extent than with the hydroxyl compounds.

There can be little doubt that if Kopp had been able, in the first place, to obtain a considerable number of pure substances of normal behaviour, such as the paraffins or their halogen derivatives, he would not have been led to the erroneous conclusions which he defended with such vigour for so many years. If we take the normal paraffins as the simplest class of organic compounds, we find that, instead of the boiling-points rising by equal intervals as the series is ascended, the rise, which is very large for the lowest numbers, becomes smaller and smaller as the molecular weight increases. This fact is, of course, now well known, and various formulæ have been suggested to reproduce these boiling-points. Thus Walker has proposed the formula $T = aM^b$, where T is the boiling-point on the absolute scale of temperature, M is the molecular weight, and a and b are constants. Ramage has this year suggested that this formula applies only to the CH_2 chain linkage, and that the influence of the terminal hydrogen atoms is considerable in the case of the lowest members, but diminishes as the chain lengthens, and becomes eventually either constant or negligible. In other words, the lower members of the series cannot be regarded as truly homologous, and that is a point which is, I think, important to bear in mind. Ramage suggests a new formula, $T = a[M(1-2^{-n})]^b$, where a is Walker's constant, 37.3775, and n is the number of carbon atoms in the molecule. He assumes, however, a constant difference for CH_2 in the case of the alcohols, the aldehydes, and the ketones, but I doubt whether the boiling-points of the last two classes of compounds are yet sufficiently well established to allow of any certain conclusions being drawn from them.

I am inclined to think that it may be useful to regard the value of Δ (the rise of B.P. for an increment of CH_2) as being mainly a function of the absolute temperature, and I would provisionally suggest the formula $\Delta = \frac{144.86}{T^{0.0148\sqrt{T}}}$

where Δ is the difference between the boiling-point, T , of any paraffin and that of its next higher homologue. Taking the boiling-point of methane as $106^\circ.75$ abs., the values for the higher members agree better with the observed temperatures than those given by Ramage's formula, as will be seen by the first table on the next column.

I do not wish, however, to lay much stress on the actual form of the equation, or on the particular values of the constants; the chief point I wish to direct attention to is that Δ may be regarded as a function of the temperature.

Suppose that we replace a terminal atom of hydrogen in each normal paraffin by chlorine, so as to form the homologous series of primary alkyl chlorides. The boiling-points of these chlorides are much higher, and the differences, Δ , are much smaller than for the corresponding paraffins, but the gradual fall in the values of Δ as the series is ascended is unmistakable. The same remarks apply to the bromides and iodides, the boiling-points being still higher and the values of Δ smaller.

But the point of chief interest appears to me to be this: if the values of Δ for the halogen derivatives are plotted against the absolute temperatures, the points for the most part fall near the curve constructed for the paraffins, and represented by the formula $\Delta = \frac{144.86}{T^{0.0148\sqrt{T}}}$. The first value of Δ is decidedly low in each case (average deviation from curve $2^\circ.7$); the later ones are rather high in nearly every case (average deviation $0^\circ.86$). Similar results are in

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Paraffin	Boiling-point (abs. temp.)				
	Observed	Calculated. Ramage	Δ	Calculated. Young	Δ
CH_4 ...	108.3	105.7	-2.6	106.75	-1.55
C_2H_6 ...	180.0	177.3	-2.7	177.7	-2.3
C_3H_8 ...	228.0	231.9	+3.9	229.85	+1.85
C_4H_{10} ...	274.0	275.6	+1.6	272.6	-1.4
C_5H_{12} ...	309.3	312.2	+2.9	309.4	+0.1
C_6H_{14} ...	341.95	343.9	+1.95	341.95	0
C_7H_{16} ...	371.4	372.3	+0.9	371.3	-0.1
C_8H_{18} ...	398.6	398.3	-0.3	398.1	-0.5
C_9H_{20} ...	422.5	422.5	0	422.85	+0.35
$\text{C}_{10}\text{H}_{22}$...	446.0	445.2	-0.8	447.85	+0.15
$\text{C}_{11}\text{H}_{24}$...	467.0	466.8	-0.2	467.35	+0.35
$\text{C}_{12}\text{H}_{26}$...	487.5	487.3	-0.2	487.65	+0.15
$\text{C}_{13}\text{H}_{28}$...	507.0	507.0	0	506.8	-0.2
$\text{C}_{14}\text{H}_{30}$...	525.5	526.0	+0.5	525.0	-0.5
$\text{C}_{15}\text{H}_{32}$...	543.5	544.2	+0.7	542.3	-1.2
$\text{C}_{16}\text{H}_{34}$...	560.5	561.9	+1.4	558.85	-1.65
$\text{C}_{17}\text{H}_{36}$...	576.0	579.0	+3.0	574.7	-1.3
$\text{C}_{18}\text{H}_{38}$...	590.0	595.7	+5.7	589.9	-0.1
$\text{C}_{19}\text{H}_{40}$...	603.0	611.9	+8.9	604.5	+1.5

general obtained with other nomologous series of compounds in which molecular association is not believed to occur, but, as will be seen from the following table, the deviations from the normal paraffin curve are greater in the case of those series the lower members of which, according to Ramsay and Shields, are characterised by molecular association.

Group	Lower members		Higher members	
	Number of values of Δ	Mean difference calculated - observed	Number of values of Δ	Mean difference calculated - observed
Alkyl chlorides ...	2	+2.70	5	-1.04
„ bromides ...	2	+1.12	5	-1.25
„ iodides ...	2	+0.52	3	-1.0
Isoparaffins ...	—	—	2	+0.57
Toluene, &c. ...	1	+0.45	3	+0.68
<i>o</i> -Xylene, &c. ...	1	+6.1	1	-0.5
<i>m</i> -Xylene, &c. ...	1	+4.25?	1	+4.0?
<i>p</i> -Xylene, &c. ...	1	-0.15	1	+0.65
Diethylbenzene, &c.	—	—	1	-0.05
Olefines $\text{H}_2\text{C}=\text{CHR}$	—	—	3	-2.35?
„ $\text{RHC}=\text{CHR}^1$	—	—	3	+0.5?
Polymethylenes ...	—	—	2	-3.85?
Ethers ...	3	+8.2	13	+1.12
Aldehydes ...	2	+2.0	4	+1.3
Hydrosulphides ...	2	+3.55	1	-0.5
Amines ...	2	+8.2	4	+1.7
Esters ...	47	+4.92	67	+1.53

Associating Substances.				
Cyanides ...	1	+12.65	4	+2.9
Nitromethane, &c. ...	2	+11.1	1	+3.85
Ketones ...	1	+6.2	3	+2.85
Fatty acids ...	2	+5.87	7	+1.58
„ alcohols ...	2	+12.87	5	+5.24

In the great majority of cases the deviations are greatest for the lowest members of a series, the calculated values of Δ being almost invariably higher than the observed, and this may perhaps be explained in the manner suggested by Ramage. I have, therefore, divided each series into two groups, the first ending and the second beginning with the lowest member of the series which contains a CH_2 group linked to two carbon atoms. Thus, of the alkyl chlorides, the first group contains CH_3Cl , $\text{CH}_3\text{CH}_2\text{Cl}$, and $\text{CH}_3-\text{CH}_2-\text{CH}_2\text{Cl}$, and the second group begins with

propyl chloride, so that all its members contain one or more C—CH₂—C groups.

In the case of the ethers, esters, and other compounds which contain two alkyl radicals, a series is regarded as homologous when one radical remains unaltered and the other increases by stages of CH₂. The variable radical only is considered in dividing the series into the two groups; thus, although propionic acid contains a C—CH₂—C group, it remains unchanged in the propionic esters, the first group of which consists of methyl, ethyl, and propyl propionate, the second beginning with the last-named ester.

Of the seventeen series of non-associating substances there are only five for which the mean difference between the calculated and observed values of Δ for the higher members exceeds 1°.5.

1. The *m*-xylene series. Here there is only one value, which, I think, is doubtful.

2. The olefines, H₂C=CHR. Here two of the three individual differences are less than 1°.5; the temperatures are all below 0°, and are somewhat uncertain.

3. The polymethylenes. The difference for pentamethylene and hexamethylene differs by less than 1° from the calculated value. The B.P. of heptamethylene appears very doubtful.

4. The amines. Differences somewhat erratic; three within 1°.5 and two within 0°.5. Octylamine and nonylamine clearly incorrect and not included.

5. The esters. Although Ramsay and Shields include these substances as non-associating, there is, I think, reason to suspect slight association.

It will be seen that the differences are greater for associating than for non-associating substances; also that they are greatest for the alcohols and least for the acids, although the factor of association is very high for both these series. In order to arrive at an explanation of these facts the effect of replacing hydrogen by chlorine may first be considered.

The boiling-point of hydrogen chloride is not yet known accurately, but it must be about -80°. Thus, by replacing an atom of hydrogen in the hydrogen molecule by chlorine the boiling-point is raised from 20°.4 abs. to about 193° abs., or about 173°. On replacing an atom of hydrogen in methane by chlorine the rise of boiling-point is from 108°.3 to 249°.3, or 141°. Ascending the series of paraffins the rise of boiling-point due to the replacement of hydrogen by chlorine diminishes rapidly at first, and then more slowly, being only 58°.5 in the case of octane. Thus the influence of the chlorine atom becomes relatively smaller as the formula weight of the alkyl group increases.

Consider, now, the effect of replacing a hydrogen atom by a hydroxyl group. In the formation of water from hydrogen gas the boiling-point is raised no less than 352°.6, from 20°.4 abs. to 373° abs., or in the ratio of 1 : 18.3; in the case of methane the rise is 221°.8, from 108°.3 to 337°.7, or in the ratio of 1 : 3.12; with octane the rise is 65°.4, from 398°.6 to 464°; and with hexadecane it is only 56°.5, from 560°.5 to 617°, the ratio being 1 : 1.10.

It will be seen that in the case of hydrogen the influence of the hydroxyl is enormously greater, and in the case of methane very much greater, than that of chlorine in raising the boiling-point, but that on ascending the series of paraffins to octane the influence of the hydroxyl group diminishes until it is little greater than that of the chlorine atom, and it is quite probable that with hexadecane it would be somewhat less. This is, no doubt, to be explained by the fact that the molecules of water and of the lower alcohols are highly associated in the liquid, but not in the gaseous state, and therefore, in order to vaporise the liquids, this molecular attraction must be overcome, and the temperature must therefore be raised. The molecular association diminishes, however, as the series of alcohols is ascended, and is probably slight in the case of octyl alcohol. If so, it would appear that the effect of the hydroxyl group—apart from association—in raising the boiling-point is not very different from, and is probably somewhat less than, that of the chlorine atom, and that the difference between the boiling-points of the lower alcohols and of the corresponding chlorides is entirely due to molecular association in the liquid state.

With the acids there is association in the gaseous as well as the liquid state, and since, according to the tables given

by Ramsay and Shields, the factor of association for a liquid fatty acid at its boiling-point is rarely greater, and in most cases is somewhat smaller, than for the corresponding liquid alcohol, the molecular attraction to be overcome on vaporisation must be considerably less for the acid than for the corresponding alcohol, and the resulting rise of boiling-point above the normal value must be less. An explanation of the very low values of Δ for the alcohols and the moderately low values for the acids is thus afforded.

It would take up far too much time and space to give full details of the boiling-points of all the compounds considered, with the observed and calculated values of Δ ; but it may, I think, be stated that the difference between the boiling-point of any non-associating organic compound which contains at least one C—CH₂—C group, and that of its next higher homologue (at any rate up to temperatures of about 300° C.), may be calculated with an error rarely exceeding 1°.5, and generally under 1°, by means of the formula $\Delta = \frac{144.86}{T^{0.045}\sqrt{V}}$. The formula seems also to be

applicable to any ester which contains at least five atoms of carbon in the variable alkyl or acyl group (the mean error for 40 values of Δ is +0°.93), and with smaller error when the number of carbon atoms is still larger; it is probably also applicable to the higher fatty acids, cyanides, ketones, and nitro-compounds.

Comparison of Molecular Volumes.

The fundamental idea on which both Kopp and Schröder based their methods of calculating the molecular volumes of organic compounds from the atomic volumes of the component elements was the constancy of the increase in molecular volume for each addition of CH₂. With regard to this point the question was greatly discussed whether the comparison should be made at the same temperature, say 0° C., or at the boiling-points of the compounds under the same pressure. Later, when Van der Waals brought forward his conception of corresponding states, it was thought probable that the comparison should be made at corresponding or equal reduced temperatures; that is to say, at temperatures which bear the same ratio to the critical temperatures. If the generalisations of Van der Waals were strictly true, the boiling-points under corresponding pressures would be corresponding temperatures, but that is not usually the case. The comparison may, therefore, be made either at equal reduced temperatures or at the boiling-points under equal reduced pressures; or, lastly, it may be made at the critical points themselves, and, thanks to the law of Cailletet and Mathias, the critical volumes can be ascertained with a great degree of accuracy.

In order to find whether the difference in molecular volume for each addition of CH₂ is really constant it is best to examine such perfectly normal substances as the paraffins, and the data for four consecutive members of the series—*n*-pentane, *n*-hexane, *n*-heptane, and *n*-octane—are fortunately available.

In the table below the molecular volumes and the

Paraffin	A		B		C		D		E	
	M. Vol.	Δ	M. Vol.	Δ	M. Vol.	Δ	M. Vol.	Δ	M. Vol.	Δ
<i>n</i> -Pentane ...	111.33	15.44	117.80	22.13	116.13	20.09	116.13	21.06	309.3	56.8
<i>n</i> -Hexane ...	126.77	15.69	139.93	22.63	136.22	20.18	137.19	21.49	366.1	60.2
<i>n</i> -Heptane ...	142.46	15.88	162.56	23.70	156.40	20.54	158.68	21.83	426.3	62.6
<i>n</i> -Octane ...	158.34		186.26		176.94		180.51		488.9	

1 Thus the observed B.P. of *n*-hexyl formate is 153°.6, and the value of Δ calculated from the formula is 22°.8, giving 176°.4 as the B.P. of the next higher homologue. This agrees very well with the observed B.P. of *n*-heptyl formate, 176°.7, but not with that of *n*-hexyl acetate, 169°.2. Again, the observed B.P. of methyl caproate (hexoate) is 149°.6, and the calculated value of Δ is 23°.0, giving 172°.6 as the B.P. of the next homologue. The observed B.P. of methyl α -naphthylate (heptoate) is 172°.1, but that of ethyl caproate is only 166°.6.

differences, Δ , for an addition of CH_2 are given under the following conditions: ¹—

- At 0°C .
- At the respective boiling points under 1 atm. pressure.
- At equal reduced temperatures (0.6396).
- At the respective boiling-points under equal reduced pressures (0.02241).
- At the respective critical points.

It will be seen that in every case there is a decided rise in the value of Δ as the series is ascended, but that the rise is relatively smallest when the comparison is made at the particular reduced temperature chosen. At higher reduced temperatures, however, it would be relatively much greater, since it is very marked at the critical point, where the reduced temperature = 1. The rise is also comparatively small at the common temperature 0° , but the comparison would not be satisfactory if a higher common temperature, say 150° , were chosen, because the coefficients of expansion differ considerably; at 150° the values of Δ would be 8.75, 13.45, and 15.38 respectively.

In the case of nine of the lower esters the values of Δ are by no means constant, whether the comparison be made at 0° , at the boiling-point, or at the critical point. The eleven values of Δ vary in the three methods between 16.34 and 18.21, 20.84 and 23.42, 54.3 and 61.7 respectively; but there is not a regular rise with increase of molecular weight.

Both Kopp and Schröder compared the molecular volumes of compounds at their boiling-points under normal pressure, but they deduced quite different values for the atomic volumes of carbon and hydrogen; it is clear, however, that as Δ varies considerably no values whatever for C and H could give accurate results, even in the case of true homologues.

Traube makes the comparison at a common temperature, usually 15° , and takes into consideration both the actual volumes of the molecules and the co-volume, which he assumes to have the same value, $24.5 (1+at)$, where $a=1/273$, for all substances. He calculates definite values for the atomic volumes of C and H at a given temperature; thus, at 15° , $\text{C}=9.9$ and $\text{H}=3.1$, or $\text{CH}_2=16.1$, so that here again the difference for CH_2 at a given temperature should be constant.

It does not appear to me that the problem has yet been completely solved, although Traube's method of calculation generally gives much better results than those of Kopp and Schröder.

Comparison of Boiling-points at a Series of Equal Pressures.

The results of this comparison are often exceedingly simple if the two substances compared are very closely related, and if there is no molecular association in either case. Taking, for example, chlorobenzene and bromobenzene, it is found that the ratio of the boiling-points (on the absolute scale of temperature) under equal pressures is constant whatever the pressure may be, or

$$\frac{T_A}{T_B} = \frac{T'_A}{T'_B} = 1.0590.$$

A similar result is obtained with the other halogen derivatives of benzene, with ethyl bromide and ethyl iodide, with ethyl acetate and propyl acetate, and some other pairs of esters; but in some cases of close relationship—for example, with ethyl formate and ethyl acetate—the ratio is not quite constant, and the formula becomes

$$\frac{T_A}{T_B} = \frac{T'_A}{T'_B} + c(T_B - T'_B),$$

where c has a very low value [0.0000417 for these two esters]. When there is no close relationship, but the molecules are not associated, the value of c is usually larger—for example, 0.0001185 for carbon disulphide and ethyl bromide.

Lastly, when there is no close relationship and the molecules of one or both substances are associated, the formula

$$\frac{T_A}{T_B} = \frac{T'_A}{T'_B} + c(T_B - T'_B)$$

¹ The atomic weights [$\text{C} = 11.97$, $\text{H} = 1$] employed in the original papers are retained.

may no longer hold, and a third term may be required, thus:

$$\frac{T_A}{T_B} = \frac{T'_A}{T'_B} + c(T_B - T'_B) + d(T_B - T'_B)^2;$$

or, in any case, the value of c becomes much higher, as with benzene and ethyl alcohol [$c=0.0008030$] or sulphur and carbon disulphide [$c=0.0006845$].

Behaviour of Liquids when Mixed Together.

There are three points to consider when two liquids are brought together—(1) their miscibility, whether infinite, partial, or inappreciable; (2) the relative volumes of the mixture and of the components; (3) the heat evolved or absorbed.

Liquids which are classed as non-miscible rarely, if ever, bear any close chemical relationship. Thus water is practically non-miscible with all hydrocarbons and with their halogen and many other derivatives; again, mercury, so far as I know, is not miscible with any liquid compound, organic or inorganic. It is true that the higher aliphatic alcohols are almost insoluble in water, although there may be said to be some chemical relationship between them, inasmuch as an alcohol may be regarded as an alkyl derivative of water. But the alcohols may also be looked upon as hydroxyl derivatives of the hydrocarbons, and, the higher the formula weight of the alkyl group, the greater is its influence, relatively to that of the hydroxyl, on the properties of the alcohol. Thus, while the lower alcohols show considerable resemblance to water—for example, in their behaviour with dehydrating agents, such as sodium, phosphoric anhydride, or lime, and in their power of uniting with metallic salts to form crystalline alcoholates corresponding to the hydrates—this resemblance diminishes as we ascend the series, and is generally not observable with the higher members.

On the other hand, the higher the molecular weight of the alcohol the closer is its resemblance to the hydrocarbon from which it is derived. This, as already mentioned, is well shown by the diminishing difference between the boiling-points of the alcohol and paraffin as the series is ascended; it may also be noted that methane was long classed as a permanent gas, while methyl alcohol is a liquid; whereas both hexdecane ($\text{C}_{16}\text{H}_{34}$) and cetyl alcohol ($\text{C}_{16}\text{H}_{33}\text{OH}$) are solids, the former melting at 18° and the latter at 50° .

It may, in fact, I think, be stated that the chemical relationship between water and methyl alcohol is fairly close, while that between water and cetyl alcohol is very distant. So, also, two adjacent members of a homologous series, such as methyl and ethyl alcohol, are more closely related than two members of widely different molecular weight, such as methyl and cetyl alcohol.

Adopting this view, it is, I believe, safe to state that liquids which are chemically closely related to each other are invariably miscible in all proportions.

As regards the relative volumes of a mixture and of its components at the same temperature, it is well known that inequality is the rule and equality the exception; and, further, that contraction is more frequently observed than expansion on admixture. So far, however, as experimental evidence is available, it appears that when the liquids are very closely related to each other the change of volume is exceedingly small. For example, with ethyl acetate and propionate in equimolecular proportions, +0.015 per cent.; toluene and ethyl benzene, -0.034 per cent.; *n*-hexane and *n*-octane, -0.053 per cent.; methyl and ethyl alcohol +0.004 per cent.; chlorobenzene and bromobenzene, no change.

When the relationship is less close the changes are usually, but not invariably, larger, and are in some cases positive, in others negative; and it is rarely possible, in the present state of our knowledge, to predict from the nature of the substances—unless one is basic and the other acidic in character—whether contraction or expansion is to be expected. Thus, when methyl alcohol is mixed with water considerable contraction occurs, although the relationship is less close than between methyl and ethyl alcohol, which expand to a minute extent on mixing.

All we can say with regard to the alcohols is that, the higher the molecular weight—or, if isomeric alcohols are

included, the higher the boiling-point—the smaller, as a rule, is the contraction on mixing with water.

Very similar remarks apply to the heat changes which occur on mixing liquids. It appears that in the case of very closely related substances these changes are exceedingly small, or negligible, as is indicated by the very minute change of temperature which has been observed, thus: ethyl acetate and propionate, $-0^{\circ}.02$; toluene and ethyl benzene, $+0^{\circ}.05$; *n*-hexane and *n*-octane, $+0^{\circ}.06$; methyl and ethyl alcohol, $-0^{\circ}.10$; chlorobenzene and bromobenzene, $0^{\circ}.00$.

It might be expected that in the case of less closely related substances contraction would be accompanied by evolution of heat and expansion by absorption of heat, but this is by no means invariably the case; for example, on mixing 40 gram-molecules of propyl alcohol with 60 gram-molecules of water there is a contraction of 1.42 per cent., but a fall of $1^{\circ}.15$ in temperature was observed. Taking the alcohols as a group, it is found that, the higher the boiling-point, the smaller is the heat evolution or the greater the absorption on admixture with water.

Properties of Mixtures.

The behaviour of two non-miscible liquids when heated together is well known, and I need only refer to the fact that the vapour pressure is equal to the sum of the vapour pressures of the pure components at the same temperature; that the boiling-point is the temperature at which the sum of the vapour pressures of the components is equal to the pressure under which the liquid is being distilled, provided that evaporation is taking place freely and the vapour is not mixed with air; and, lastly, that the composition of the vapour is independent of that of the liquid (so long as both components are present in sufficient quantity), and is expressed by the equation $\frac{x_A}{x_B} = \frac{P_A D_A}{P_B D_B}$, where x_A and x_B are the relative weights of the two components in the vapour, P_A and P_B their vapour pressures at the observed boiling-point, and D_A and D_B their vapour densities.

The vapour pressure, boiling-point, and vapour composition, then, can be calculated for non-miscible liquids, and it has been stated that such liquids have never any close chemical relationship, and are usually not related at all.

On the other hand, it has been mentioned that when the chemical relationship is very close the liquids are invariably miscible in all proportions, and that there is very little, if any, volume or heat change on admixture.

So, also, the vapour pressure and boiling-point of a mixture of closely related liquids are easily ascertained from those of the pure components, and the composition of the vapour bears a simple relation to that of the liquid.

The vapour pressure of the mixture is given, at any rate with a very close approach to accuracy, by the equation

$$P = \frac{mP_A + (100 - m)P_B}{100},$$

where P , P_A , and P_B are the vapour pressures of the mixture and of the components, A and B, at the observed boiling-point, and m is the molecular percentage of A.

Van der Waals concluded from theoretical considerations that this relation should be true when the critical pressures are equal and the molecular attractions agree with the formula proposed by Galitzine and by D. Berthelot, $a_{1,2} = \sqrt{a_1 a_2}$, where $a_{1,2}$ represent the attraction of the unlike molecules and a_1 and a_2 the respective attractions of the like molecules. That is certainly the case with chlorobenzene and bromobenzene, which, as already mentioned, show no heat or volume change on admixture, for the maximum difference between the observed and calculated pressure in three experiments was less than 0.1 per cent.

But the relation is, at any rate, very nearly true for closely related substances when the critical pressures are not equal, for in the case of methyl and ethyl alcohol the difference between the observed and calculated pressure was within the limits of experimental error, and with four other pairs of closely related substances the greatest mean difference (for three readings each) was only 0.6 per cent. It is not, however, as Speyers suggested, true for all non-

associated substances, whether closely related or not; indeed, chemical relationship seems to be much more important than the state of molecular aggregation, for the relation is true for methyl and ethyl alcohol, while it is altogether untrue for benzene and hexane.

The boiling-point of a mixture of closely related liquids may be ascertained from the vapour pressures of the components, but not so simply as in the case of non-miscible liquids, because the boiling-point depends on the composition of the liquid.

In order to calculate the boiling-points of all mixtures of two closely related liquids under normal pressure we should require to know the vapour pressure of each substance at temperatures between their respective boiling-points under that pressure. Thus, chloroform boils at $132^{\circ}.0$, and bromobenzene at $156^{\circ}.1$, and we must be able to ascertain the vapour pressure of each substance between 132° and 156° .

The percentage molecular composition of mixtures which exert a vapour pressure of 760 mm. must then be calculated at a series of temperatures—say every two degrees—between

these limits by means of the formula $m = 100 \cdot \frac{P_B - P}{P_B - P_A}$, where, in this case, $P = 760$.

Lastly, the molecular percentages of A, so calculated, must be mapped against the temperatures, and the curve drawn through the points will give us the required relation between boiling-point and molecular composition under normal pressure. In the case of six pairs of closely related liquids the greatest difference between the observed temperature and that read from the curve constructed as described was $0^{\circ}.27$.

For liquids which are not closely related the differences are usually much greater, and particular mixtures of constant (minimum or maximum) boiling-point are not unfrequently met with, especially when the molecules of one or both substances are associated in the liquid state.

The formula for the composition of the vapour from a mixed liquid suggested independently by Berthelot and by

Wanklyn, $\frac{x_A}{x_B} = \frac{W_A P_A D_A}{W_B P_B D_B}$, (where x_A and x_B , P_A and P_B , D_A

and D_B , have the same meaning as in the equation for non-miscible liquids, and W_A and W_B are the relative weights of the two components in the liquid mixture), was shown by F. D. Brown to be incorrect, and he proposed

the simpler formula, $\frac{x_A}{x_B} = c \frac{W_A}{W_B}$, where c is a constant

which does not differ greatly from $\frac{P_A}{P_B}$. The subject was

investigated mathematically by Duhem and by Margules, and experimentally and mathematically by Lehfeldt and by Zawidski. The two last-named observers deduced workable formulæ from the fundamental equation of Duhem and Margules, and it is noticeable that both Lehfeldt's and Zawidski's formulæ, in their simplest form, become identical with Brown's. Zawidski's, however, assumes the form $\frac{x_A}{x_B} = \frac{P_A}{P_B} \cdot \frac{W_A}{W_B}$. This formula is certainly not, as a

rule, true for mixtures of liquids which are not closely related; but, on the other hand, in the very few cases examined the equation $\frac{x_A}{x_B} = c \cdot \frac{W_A}{W_B}$ appears to hold for those mixtures for which the equation

$$P = \frac{mP_A + (100 - m)P_B}{100}$$

is true; that is to say, generally, for closely related liquids.

The question, however, whether $c = \frac{P_A}{P_B}$ is an open one; but it is interesting to remark that if this equality holds it should be possible in many cases to calculate the vapour pressure at any temperature, the boiling-point under any pressure, and the composition of the vapour, of any mixture of two very closely related liquids, if the boiling-point of one of them under any one pressure, and the vapour pressures of the other within sufficiently wide limits of temperature, are known. For the boiling-points on the absolute scale of the two liquids at the same pressure bear a constant ratio to

each other, or $\frac{T_A}{T_B} = \frac{T'_A}{T'_B}$; hence the vapour pressures or boiling-points of one substance can be calculated if those of the other are known. Again, from the vapour pressures of the pure substances we can calculate the vapour pressures and the boiling-points of all mixtures; and, lastly, if $c = \frac{P_A}{P_B}$,

we can make use of Brown's formula, $\frac{x_A}{x_B} = c \frac{W_A}{W_B}$, to calculate the composition of the vapour from all mixtures without carrying out special experiments to find the value of c . It is, therefore, a matter of considerable interest to ascertain whether c is really equal to $\frac{P_A}{P_B}$ or not.

When the equation

$$P = \frac{mP_A + (100 - m)P_B}{100}$$

does not hold good, a modification of Brown's formula, or that of Lehfelt, or of Zawidski, must be employed to calculate the vapour composition, and the constants for those formulæ must first be determined experimentally.

Other physical properties, such as the refractive power of mixtures, might be considered, but I will only refer to the critical temperature and pressure. In 1882 Pawlewski stated that the critical temperature of a mixture could be calculated from those of the components by the formula

$$\theta = \frac{m\theta_A + (100 - m)\theta_B}{100},$$

where m is the percentage by weight of A; and G. C. Schmidt, in 1891, carried out experiments to test the correctness of the statement, purposely choosing substances of widely different physical properties. The differences between the calculated and observed temperatures were not, as a rule, very great, rarely exceeding 4° , and Schmidt considered that they might, to some extent, be accounted for by partial decomposition of one or other component.

Such determinations are, however, liable to serious errors. It is exceedingly difficult to fill a tube with the required amount of a liquid mixture of known composition quite free from air, and although the composition of the very small amount of liquid employed might be determined after the experiment from its specific refractive power, it would be necessary to know the specific refractive powers of the two components and of mixtures of them. Schmidt does not state how he prepared his mixtures and determined their composition.

Again, when a liquid mixture is heated in a sealed tube, fractionation goes on, so that the more volatile component tends to accumulate in the upper part of the tube, leaving the less volatile component in excess below, and unless a stirring arrangement, such as that devised by Kuenen, is employed, many hours would elapse before complete admixture by diffusion took place at the critical point.

By far the most important and accurate experiments on this subject have been carried out by past or present pupils of Prof. Kamerlingh Onnes, notably by Prof. Kuenen; and it is quite certain that the formula of Pawlewski cannot be generally true for mixed liquids, for, just as we may have mixtures of minimum or maximum boiling-point, so also, as Kuenen has shown, mixtures of minimum or maximum critical temperature may exist. Thus the critical temperature of carbon dioxide is $31^\circ.1$, and of ethane, $32^\circ.0$, but that of a mixture containing 30 molecules per cent. of carbon dioxide is $18^\circ.8$. The question remains, however, whether some such law as that proposed by Pawlewski may not hold good for closely related substances. In certain cases, when the relationship is very close (for example, C_6H_5Cl and C_6H_5Br), the critical pressures are equal, or very nearly so, and it seems probable that the critical pressure would be the same for any mixture as for the components. Such a case as this would be likely to give the simplest possible relation between the critical temperatures of a mixture and those of its components; and although the critical temperatures of these substances are inconveniently high, there are, no doubt, others which might be employed—perhaps ethyl chloride and bromide, or possibly carbon dioxide and carbon disulphide. I imagine, however, that

Pawlewski's formula would be more likely to hold if m represented the *molecular percentage*, and not the percentage by weight of A.

In the case of homologous compounds, paraffins, ethers, esters, and so on, the critical pressures are not equal, and it would be necessary to find whether the critical pressures of mixtures are represented by the formula

$$P = \frac{mP_A + (100 - m)P_B}{100}$$

(where m is the molecular percentage of A), and also whether any such simple formula is applicable to the critical temperatures.

Kuenen has made some observations with mixtures of ethane and butane containing 2.5 and 5 molecules per cent. of butane, and at the conclusion of his paper he says: "If there was a simple law connecting the critical constants of mixtures with those of the constituents, we might calculate the constants for the second substance [those of the first being known]. But such is not the case. Pawlewski's law that the critical temperature is proportional to the composition, expressed in weight units, is very inaccurate, the deviations being sometimes considerable in both directions."

It would, I think, be of great interest if Prof. Kuenen could find time to carry out further experiments with mixtures of ethane and butane in order to settle this point, or, perhaps, with *n*-hexane and *n*-octane, both of which can be more easily obtained in a pure state.

From what has been said it may be concluded that, in order to ascertain the normal behaviour of pure substances under different conditions, or to find the simplest relations between the boiling-points, molecular volumes, or other physical constants of a series of substances, or, again, to ascertain the normal behaviour of substances when mixed together, and the properties of the mixtures as compared with those of the components, it is undoubtedly advisable—at first, at any rate—to confine our attention to substances of which the molecules show no signs of association in either the gaseous or liquid state.

In the case of mixtures it is also best to begin with substances which are chemically closely related to each other.

SECTION C.

GEOLOGY.

OPENING ADDRESS BY AUBREY STRAHAN, M.A., F.R.S.,
PRESIDENT OF THE SECTION.

It is forty-two years since the British Association last met in Cambridge, and we may turn with no little interest to the record of what was taking place at a date when the science of Geology was still in its infancy, and in a University where its promise of development was first recognised. Dr. John Woodward, the founder of the Woodwardian Chair, had been dead 176 years, but his bequest to the University had not long begun to bear fruit, for the determination to house suitably the collection of fossils and to provide for the reading of a systematic course of lectures was not arrived at until 1818. In that year Adam Sedgwick, on his appointment to the Woodwardian Chair, began a series of investigations into the geology of this country, which made one of the most memorable epochs in the history of British Geology. At the Cambridge meeting of 1862 he had therefore held the professorship for forty-four years, a period sufficient to spread his reputation throughout the civilised world as one of the pioneers of geological science.

Towards the close of his life Sedgwick gave expression to the objects which he had had in view when he accepted a professorship in a science to which he had not hitherto specially devoted his attention. "There were three prominent hopes," he writes, "which possessed my heart in the earliest days of my Professorship. First, that I might be enabled to bring together a Collection worthy of the University, and illustrative of all the departments of the Science it was my duty to study and to teach. Secondly, that a Geological Museum might be built by the University, amply capable of containing its future Collections; and lastly, that I might bring together a Class of Students who would listen to my teaching, support me by their sympathy, and help me by the labour of their hands."

We, visiting the scene of his labours more than thirty years after he wrote these words, witness the realisation of Sedgwick's hopes. The collection is not only worthy of the University, but has become one of the finest in the kingdom. It is housed in this magnificent memorial to the name of Sedgwick, on the completion of which I offer for myself, and I trust I may do so on behalf of this Section also, hearty congratulations to the Woodwardian Professor and his staff. Finally, I may remind you that at this moment the Directorship of the Geological Survey and the Presidential Chair of the Geological Society are held by Cambridge men; that the sister University has not disdained to borrow from the same source; and lastly, that it is upon Cambridge chiefly that we have learned to depend for recruiting the ranks of the Geological Survey, as proofs that Cambridge has maintained her place among the foremost of the British schools of Geology.

Though he had taken a leading part at former meetings of the Association, Sedgwick's advanced age in 1862 necessitated rest, and this Section was deprived to a great extent of the charm of his presence. It benefited, however, in the fact that the Presidential Chair was occupied by one of his most distinguished pupils. Jukes was one of those men the extent of whose knowledge is not readily fathomed. It has been my experience, and probably that of many others in this room, to find that some conclusion, formed after prolonged labour and perhaps fondly imagined to be new, has been arrived at years before by one of the old geologists. Such will be the experience of the man who follows Jukes's footsteps. Turning to his Address given to this Section in 1862, we find much of what is now written about earth-movement and earth-sculpture forestalled by him, with this difference, however, that whereas the custom is growing of using a phraseology which may sometimes be useful, but is generally far from euphonious, and not always intelligible, he states his arguments in plain, forcible English.

It may raise a smile to find that Jukes thought it necessary in 1862 to combat the view that deep and narrow valleys had originated as fissures in the crust of the earth, and that the Straits of Dover must have been formed in this way, because the strata correspond on its two sides. But we shall do well to remember that the smile will be at the public opinion of that day, and not at Jukes himself. In no branch of Geology have our views changed more than in the recognition of the potency of the agents of denudation. In 1862 it was necessary to present preliminary arguments and to draw inferences which in 1904 may be taken as granted.

The evidences of the prodigious movements to which strata have been subjected, and of the extent to which denudation has ensued, cannot fail to strike the most superficial observer. Both mountain and plain present in varying degree proof that sheets of sedimentary material originally horizontal are now folded and fractured. But after a momentary interest aroused by some example more striking than usual, glimpsed, it may be, from a train-window, the subject is probably dismissed with an impression that such phenomena are due to cataclysms of a past geological age, and have little concern for the present inhabitants of the globe. These stupendous disturbances, it might be argued, can only have taken place under conditions different from those which prevail now. We are familiar with mountain-ranges in which their effects are conspicuous; we have carried railways over or through them and have been troubled by no cataclysmic movements of the strata. Apparently the rocks have been fixed in their plicated condition, and are liable to no further disturbance. Parts of the world, it is true, are subject to earthquakes accompanied by fissuring and slight displacement of the crust, but not even in earthquake regions can we point to an example of such thrusting and folding of the strata being actually in progress as have taken place in the past. Nor, again, can volcanic activity be appealed to, for some of the most highly disturbed regions are devoid of igneous rocks. Volcanic eruptions are more probably the effect than the cause of the disturbances of the crust. Nowhere in the world therefore, it will be said, can we see strata undergoing such violent treatment as they have experienced in the past. How, then, can we dispute the inference that the forces by which the folding was produced have ceased to operate?

Before accepting a conclusion which would amount to

admitting that the globe is moribund and that the forces by which land has been differentiated from sea have ceased to act, we shall do well to look more closely into the history of the earth-movements to which any particular region has been subjected. The investigation is one which calls for the most intimate knowledge of the geological structure, and, as time will admit of my dealing with a small area only, I shall confine my observations to England and Wales, selecting such facts as have been established beyond dispute.

At the outset of the investigation we find reason to conclude that the movements, so far as any one region is concerned, have been intermittent. Evidence of this fact is furnished wherever any considerable part of the geological column is laid open to view. Sheets of sediment, aggregating perhaps thousands of feet in thickness, have been laid down in conformable sequence, all bearing evidence of having been deposited in shallow seas. The inference is inevitable that that period of sedimentation was a period of uninterrupted subsidence. But sooner or later every such period came to an end. Compression and upheaval took the place of subsidence, and the strata lately deposited were plicated and brought within the reach of denudation. Illustrations of the recurrence of these movements abound, and I need dwell no further upon them than to remark that movements of subsidence and upheaval may be seen to have alternated wherever opportunity is afforded for observation.

On extending our observations we are led to infer that the movements of the crust were developed regionally, not universally. The areas of subsidence, for example, evidenced by the marine formations, had their limits, though those limits did not coincide with the shores of existing seas, nor has reason been found to believe that the proportion of land to sea has varied greatly in past times. The limits of the area affected by any one movement of upheaval are more difficult to determine, but the effects were manifested in the crumpling up of comparatively narrow belts of country, and are easy of recognition.

Further than this, we ascertain that the movements of one region were not necessarily contemporaneous with those of adjoining regions. The forces operating upon the crust of the earth came into activity in different places at different times, and, while some continental tracts have been but little disturbed from early geological times, there are parts of the globe which have been the scene, so to speak, of almost ceaseless strife. Among the latter we may include the British Isles.

These are commonplaces of Geology, and I mention them merely to emphasise the fact that the geological structure of these islands is the result of movement superimposed upon movement. Obviously, therefore, in order to gain a comprehensive view of the operations which were in progress in any one region during any one epoch, we have to find some means of distinguishing the movements of that epoch and of eliminating all which preceded or followed it. This, briefly, is the problem which has engaged the attention of geologists for many years past, and upon which I propose to touch.

The determination of the age of a disturbance is seldom easy, and among the older Palæozoic rocks is often impossible; but at the close of the Carboniferous period, during the great continental epoch which led to and followed upon the deposition of the Coal Measures, there came into action a set of movements of elevation and compression which generally can be distinguished both from those which preceded them and from those which have been superimposed upon them. The distinction depends upon the determination of the age of the rocks affected by the movements. For example, a movement by which the latest Carboniferous rocks have been tilted from their original horizontal position is obviously post-Carboniferous. On the other hand, if Permian rocks lie undisturbed upon those tilted Carboniferous rocks it is equally obvious that the movement was pre-Permian. Now it happens that earth-movements of the date alluded to were particularly active in the British Isles, and played an important part in shaping the platform on which the Permian and later rocks were laid down. Though they have been more completely explored than others in the working of coal, their further investigation is of the greatest economic importance. I have attempted, therefore, briefly to sketch out the principal lines along which earth-movements of that age came into operation in England,

premising, however, that by Permian I mean the Magnesian Limestone series, and not the "Permian of Salopian type," which is now known to be partly of Triassic but principally of Carboniferous age. In the course of the investigation we shall find reason to conclude that several at least of the movements followed old axes of disturbance, lines of weakness dating from an early period in the history of the habitable globe; and, again, that some of the latest disturbances of which we have cognisance were but renewals of movement along the same general lines.

One of the most clearly proved examples of pre-Permian faulting in the Carboniferous rocks occurs in the Whitehaven Coalfield. The fault forms the south-eastern limit of the Coal Measures, and has been precisely located for a distance of six miles. In its course towards the south-west it passes under five outliers of Permian rocks, and finally is lost to sight under the Permian and Trias of St. Bees. The dislocation in the Carboniferous rocks amounts to about 400 yards, but the Permian rocks have not been even cracked; though broken and displaced by numerous faults of later date, they pass undisturbed over this great dislocation, the movement along it obviously having ceased before they were deposited. This fault forms part of the upheaval which brought the older rocks of Cumberland and Westmorland to the surface, and in that sense it may be said to form the north-western frontier of the Lake District.

On the north-eastern side also of the Lake District the Permian rocks rest upon uptilted Carboniferous strata, but the axis of upheaval runs in a north-north-westerly direction and defines what we may regard as the north-eastern frontier. Along this frontier much movement has taken place in post-Permian times, but the unconformable relations of the Permian and Carboniferous rocks enable us to distinguish that part of the tilting which intervened between the two periods. On the south-eastern frontier also the Carboniferous rocks had been upheaved and denuded before the Permian sandstones were laid down. A huge fault, along which Carboniferous rocks have been jammed from the east in a multitude of plications against Silurian, runs from Kirkby Stephen by Dent to Kirkby Lonsdale, and thence trends south-eastwards by Settle. It is highly probable, though it has not been proved, that this fault is of pre-Permian age. That the Pendle axis which upheaves the Lower Carboniferous rocks between Settle and Burnley is pre-Permian is placed beyond doubt by the fact that an outlier of Permian rests upon the denuded crest of the anticline near Clitheroe.

The south-western frontier is defined by a still more marked unconformable overlap by the Permian strata, which here pass over the edges of the lowest members of the Carboniferous series and come to rest upon the Lake District rocks.

We have thus defined the sides of an oblong tract which was upheaved in the period we are considering. The older rocks forming the northern part of that tract had already had imposed upon them a dominant north-easterly strike by a pre-Carboniferous movement of great energy. As a result also of that and other movements they had been subjected to vast denudation, not only in the Lake District, but throughout the north-west of England generally. But while it is doubtful whether any of the physical features then produced have survived, it seems to be beyond dispute that it was in consequence of the pre-Permian movements that the older rocks of the Lake District were freed from their Carboniferous covering, and that to this extent the district may be said to have been blocked out in pre-Permian times. The detailed sculpturing resulted from later movements, with which we are not now concerned.

During this same period there rose into relief that part of the Pennine axis which runs between Lancashire and Yorkshire. The doming up of the Lower Carboniferous rocks and the wildness of the moorlands which characterise their outcrops have impressed all who have had occasion to cross from the one populous coalfield to the other, and have gained the name of the "backbone of England" for this antichinal axis. Whether, however, it can be regarded as one axis or as the result of several movements is doubtful, but not material for our present purpose. Regarded as a geological structure it is not continuous with that part of

the Pennine axis which runs along the north-eastern frontier of the Lake District.

Passing westwards from the Pennine axis we cross the deep and broad Triassic basin of Cheshire, which may be regarded as the complement of the dome of elevation of Derbyshire. To the west of this, again, we reach a part of North Wales which was more or less shaped out by the earth-movements which came into action between the Carboniferous and Permian periods. Two leading faults traverse the district. The one runs in a north-north-westerly direction across Denbighshire and introduces that little bit of "Cheshire in Wales" known as the Vale of Clwyd. Though there has been some later movement along this fault, it was in the main pre-Triassic, which statement, in view of the perfect conformity between the Permian and Trias, amounts to saying that it was pre-Permian. The other passes across Wales in a north-easterly direction along the Dee Valley at Bala, and reaches the Triassic basin between Chester and Wrexham. The date of this fault has not been worked out in detail, but the fact that it is associated with a pre-Triassic anticline, where it reaches the Triassic margin, proves that it is in part at least of pre-Triassic age. In Anglesey also there has been strong post-Carboniferous folding in the same N.E.-S.W. direction.

It is to be noticed, further, that the Carboniferous rocks maintain their characters to their margins on the flanks of the Clwydian Hills and other ranges of Silurian rocks in North Wales. Both along the coast, and even in a little outlier preserved near Corwen by an accident of faulting, they show a persistence of type and of detail in sequence which could hardly have been maintained had the Silurian uplands existed in Carboniferous times. The inference that the uplands of Denbighshire and Flintshire are the result of post-Carboniferous upheaval is strengthened by the fact that the Carboniferous rocks reposing on their flanks are tilted at an angle which would carry them over their tops. This part of North Wales, therefore, presents a history corresponding in its main events with that of the Lake District. It had undergone elevation and denudation in pre-Carboniferous times on a scale so vast that rocks showing slaty cleavage and other indications of deep-seated metamorphism had been laid bare. But in both cases it was in consequence of the post-Carboniferous movements that the leading physical features as they exist today began to take shape.

In both these regions pre-Carboniferous movements had been extremely active. For example, an axis of compression and upheaval ranges from N.E. to S.W., involving the Lake District, the Isle of Man, and Anglesey. It belongs to the Caledonian system of disturbances which is developed on a large scale further north, and which sufficed here to cause slaty cleavage and presumably the extrusion of the Shap granite. I mention this pre-Carboniferous axis to point out that it offers an explanation of the direction taken by the post-Carboniferous disturbances of Whitehaven, Pendle, Anglesey, and possibly Bala. With the exception of the last-named they lie well within the region affected, and alone among the post-Carboniferous axes take that particular direction.

The Pennine axis ends as a physical feature in South Derbyshire and North Staffordshire on the margin of a deep channel filled with Triassic marl, which extends westwards from Nottingham into Shropshire. In this part of England there springs into existence a remarkable series of disturbances tending to radiate southwards. The westernmost of these is the great fault which forms the western boundary of the North Staffordshire Coalfield. Recent work by Mr. W. Gibson has shown that the vertical displacement of the Coal Measures amounts to no less than 900 yards, but that it is far less, though recognisable, in the Trias, proving that the disturbance was in the main pre-Triassic. The fault ranges from Macclesfield in a south-south-westerly direction, is lost to view under the Trias near Market Drayton, but it is recognisable further on in the great dislocation which passes along the western side of the Wrekin, and thence through Central Shropshire by Church Stretton to Presteign in Radnorshire, and thence into Brecknock.

The second is the Apedale Fault of the North Staffordshire Coalfield. In working the coal this disturbance has

been found to possess the structure of a broken monocline, a fold with fracture such as may be regarded as an early stage in the formation of an overthrust from the east. It runs through the coalfield in a direction slightly east of south, and then passing under the Trias of Stafford ranges for Wolverhampton and Stourbridge. This fault is mainly pre-Triassic, but what Mr. Gibson believes to be a continuation of it, following the same direction as far south as Hanbury, certainly effects a great movement in the Trias.

The third disturbance runs on the east of the Forest of Wyre Coalfield in a direction a little west of south. Here, as I learn from Mr. T. C. Cantrill, the thrust from the east is obvious, for Old Red Sandstone has been pushed from that direction against and even over Coal Measures, while the strata have been forced up into a vertical position for some miles. In South Staffordshire all the Carboniferous rocks, including the "Salopian Permian," are involved in this and the previously mentioned movement, proving that both disturbances were of post-Carboniferous date.

Traced southwards this disturbed belt leads to Abberley, and there connects itself with the well-known Malvernian axis. The broken belt known by that name runs north and south, and may be followed almost continuously from Worcestershire to Bristol. It presents evidence of having been a line of weakness through a large part of the world's history, as shown by Prof. Groom, and of having yielded repeatedly to earth-stresses; but there is seldom difficulty in distinguishing the movements which were effected during the period under consideration. For example, near and south of Abberley the Coal Measures are clearly involved in a thrust from the east, which was sufficiently energetic to turn over a great belt of Old Red Sandstone and other rocks beyond verticality for some miles. Further south, again, among repeated proofs of the ridging up of the old axis in several pre-Carboniferous periods, we find evidence of post-Carboniferous elevation along the same general line. Throughout this same region there has been also post-Triassic dislocation, which, however, is on a comparatively small scale. That the Carboniferous rocks were greatly disturbed before the Trias was laid down is proved by the great unconformity between the two formations.

The Malvernian axis continues southwards by Newent, but perhaps with diminishing intensity. On its west side a broad syncline rolls in the tract of Carboniferous rocks which underlies the Forest of Dean. The syncline trends north and south, and is shown to be of pre-Triassic age by the fact that the Triassic strata on the banks of the Severn do not share in the synclinal structure. Here we must leave the Malvernian axis for the present.

The fourth disturbance ranges along the Lickey Hills, which, diminutive as they are, tell a story of great geological significance. They range in a south-south-easterly direction, and in the fact that they are formed of extremely ancient rocks furnish evidence of immense upheaval. From the relations of these ancient formations to one another we may gather also that the upheaval was due to a recurrence of movement along the same axis at more than one geological date, but at the same time we find no difficulty in distinguishing that part of the movement which took place between Carboniferous and Triassic times, for the Coal Measures are tilted up on end along the flanks of the axis, while the Trias passes horizontally over all the tilted rocks. A clue to the southward extension of the axis under the Secondary rocks is furnished by some faulting as far as Redditch, here also there having been a renewal of movement on a small scale in post-Triassic times.

The fifth disturbance runs through Warwickshire, and includes the low ridge of ancient rocks which ranges through Atherstone and Nuneaton in a south-easterly direction. About fifteen miles to the north-east Archæan rocks form the parallel ridge or series of ridges of Charnwood Forest, while the intervening space is overspread by Trias, resting partly on Carboniferous and partly on older strata. The structure of the Carboniferous and older strata is dominated by what is known as the Charnian movement, which includes disturbances of several ages ranging in a south-easterly direction. That part of the movement which was post-Carboniferous is identifiable by the fact that Coal Measures are tilted on either side of the ridges of old rocks. They once overspread both ridges, but were

removed by denudation as a consequence of upheaval before the Trias was deposited. It has been found also in working the coal, as I am informed by Mr. Strangways, that there are large faults having the south-eastward or Charnian direction which shift the Coal Measures, but do not break through the overlying Trias. The evidence, therefore, of a great Charnian movement having taken place during the period under consideration is conclusive. The disturbance ranges as a whole in the direction of Northampton, where in fact borings have reached the Charnwood rocks at no great depth.

The five great disturbances which I have briefly indicated tend to converge northwards, but their exact connection with the Pennine axis is not known. What may be only a part of that axis trends for Charnwood through a tract of Lower Carboniferous rocks exposed at Melbourne, between the Yorkshire and Leicestershire Coalfields, but the Triassic channel I have already mentioned intervenes, and the structure of the rocks underlying the red marl is unknown. The channel itself appears to be of Triassic age, for not only is the depth of marl in it suggestive of its having been a strait in the Triassic waters, but its northern margin has been found by Mr. Gibson to coincide with, and perhaps to have been determined by, faults known to be mainly of pre-Triassic age. One of these, with a downthrow of 400 yards to the south, runs from Trentham through Longton, and south of Cheadle, while another ranges from near Nottingham to the north of Derby.

We come now to the south-west of England, where we find striking proofs of a still more energetic movement than any yet mentioned having intervened between the Carboniferous and Triassic periods. The central part of the Armorican axis, as it has been called, after the ancient name of Brittany, trends nearly east and west, and keeps to the south of our South Coast; but we have opportunities in Devon and Cornwall of seeing some of the stupendous effects produced along its northern side. A belt of country measuring some 130 miles in width has been completely buckled up. Slaty cleavage was superimposed upon the intricate folds into which the strata were being thrown, while after or towards the close of these phenomena granite was extruded at several points along the belt of disturbance, a little north, however, of the line along which the oldest rocks were brought up to the surface. In Devon the Culm-measures are fully involved in the movement, but on the other hand the Permian strata, while containing fragments of the cleaved and metamorphosed rocks, are themselves wholly free from such structures. The age of the folding, cleavage, and extrusion of the granite is thus definitely fixed as having been subsequent to the deposition of the Culm-measures, but previous to that of the Permian rocks.

But we may fix the age still more closely. A broad syncline of Carboniferous rocks traverses Mid-Devon, and is succeeded northwards by an anticline and by an extrusion of granite at Lundy Island, the age of which, however, has not yet been definitely ascertained. Still further north in a series of folds and overthrusts which traverse the southern margin of South Wales we can recognise the last effects of the great Devonshire movement at a distance of not less than 130 miles from the central axis, the ground-swell, so to speak, subsiding as it receded from the distant storm-area. Here the higher Carboniferous rocks are involved, and thus prove that this part at least of the Armorican disturbance was of post-Carboniferous age.

In Dorset, Somerset, and Gloucestershire the Palæozoic rocks pass eastwards under Secondary formations, and are seen no more in the south of England. That the disturbance continues, however, is inferred from the fact that it has been traced across a large part of the continent of Europe in the one direction and across the south of Ireland in the other. The determination of its position therefore, and especially of the effects of its intersection with the Midland disturbances, is of the greatest importance in view of the possible occurrence of concealed coalfields under the Secondary rocks. One such intersection is open to observation.

The Malvern and Devonshire disturbances intersect in Somerset. On investigating their behaviour as they

approach we may notice in the first place that the subsidiary axes which form the northernmost part of the Devonshire disturbance in South Wales die away one after the other towards the east. Thus an east and west disturbance at Llanelly runs a few miles and disappears. The more important Pontypridd anticline, which traverses the centre of the coalfield, fades away near Caerphilly, while the coalfield itself terminates a little further east, its place on the same line of latitude being taken by the Usk anticline, which trends southwards and south-westwards. So far it might be inferred that the east and west folds die away on approaching the north and south Malvernian axis. But the Cardiff anticline, which lies south of and was more energetic than those mentioned, crosses the Bristol Channel and, emerging on the other side in a complicated region near Clevedon and Portishead, passes to the north of Bristol and holds its course right across the coalfield at Mangotsfield. The coalfield, however, lies in what is part of the Malvernian disturbance, for it occupies a syncline running north and south along the west side of the main axis of upheaval. Though the interruption is local and the strata recover their north and south strike to the south of it, yet the east and west axis obviously holds its course right through the Malvernian structure.

Still further south in the direction in which the east and west movements gradually increase in energy a series of sharp folds is well displayed in the coast of South Wales and in an island in the Bristol Channel, ranging for that part of the east and west disturbance which is known as the Mendip axis. This name has been applied to a series of short anticlines which are arranged *en échelon* along a line ranging east-south-east, but each of which runs east and west. Among them we may distinguish the Blackdown anticline, the Priddy anticline, the Penhill anticline, north of Wells, and the Downhead anticline, north of Shepton Mallet. With one exception they all die out eastwards after a course of two to ten miles, but the Downhead anticline holds its course into the Malvernian disturbance, the two engaging in a prodigious *mêlée* south of Radstock. From that much shattered region the Downhead anticline emerges, but the Malvernian axis is seen no more, and, so far as can be judged under the blanket of Secondary rocks, comes to an end.

Mention has been made of the fact that many of the subsidiary east and west folds die away on approaching the Malvernian axis. In a general way we may attribute their disappearance to the influence of the north and south movement, for it is commonly to be observed in these great belts of disturbance that they are composed of a number of parallel anticlines or elongated domes of upheaval, constantly replacing one another; it is a common feature also that these subsidiary folds replace one another not exactly in the direction in which they point, but that they lie *en échelon* along a line slightly oblique to it. The behaviour of the South Wales and Mendip folds is in accordance with these observations, and may be taken to indicate that the effects of the east and west disturbance reached further north in South Wales than they did in Somerset, or, in other words, that they failed to penetrate as far into the region where north and south movements were in progress as in the region where there were no movements of that direction.

The fact that the east and west folds keep their course across the north and south wherever the two actually meet comes out prominently, and supports the inference that they dominate the structure of the Palæozoic rocks which lie hidden beneath the Secondary rocks of the south and south-east of England. Somewhere under this blanket of later formations the east and west axis presumably intersects the other disturbances which traverse the Midlands. To ascertain where and how the intersections take place will be going far towards locating any concealed coalfields which may exist; but the knowledge can be obtained only by boring, and the number of such explorations as yet made is wholly insufficient. The majority have been made in search of water, and have been stopped as soon as a supply was secured. Near Northampton the older rocks were reached at a small depth on what is believed to be the underground continuation of the Charnian axis, and a boring at Blethley traversed what is thought to have been a great boulder of Charnian rock, suggesting that the axis

is not far off; but with these exceptions the counties of Oxford, Buckingham, Bedford, Huntingdon, Cambridge, and Norfolk are unknown ground. Yet under these counties the axes must run if they keep their course. Where exposed at the surface each post-Carboniferous syncline between two axes contains a coalfield. It remains to future exploration to ascertain whether similar conditions hold good under the Oolitic and Cretaceous areas of Central England.

In speaking of the north and south disturbances I have in more than one case stated that the post-Carboniferous movement was but a renewal of activity along an old line of disturbance. The fact is proved by the unconformities visible among the pre-Carboniferous rocks, and it is important for the reason that the geography of this part of the globe at the commencement of the Carboniferous period had been determined by these movements. It has long been known, for example, that the parts of the counties of Stafford, Warwick, and Leicester traversed by the axes of upheaval were not submerged till late in the Carboniferous period. On the other hand, some of the area lying immediately west of the Malvernian axis was submerged at an earlier date, as is shown by the existence of Carboniferous Limestone at Cleobury Mortimer and, in greater development, in the Forest of Dean. The borings near Northampton also proved the presence of Carboniferous Limestone, a fact which is in favour of the occurrence of concealed coalfields, in so far as it indicates that the whole Carboniferous series may have once existed there. It is remarkable that none of the borings in the south and east of England have touched Carboniferous Limestone, all having passed into older or newer rocks. The existence of that formation is neither proved nor disproved.

The determination of the age of these disturbances and a discussion of the pre-Carboniferous geography may seem at first sight to be only of scientific interest, but that problems of great economic importance are involved has been shown recently. It has long been known that the principal coal-seam of South Staffordshire deteriorates westwards as it approaches the pre-Carboniferous ridge evidenced in the neighbourhood of Wyre Forest. There seemed, however, to be no theoretical reasons why it should not keep its characters on either side of the fault which forms the western boundary of the South Staffordshire Coalfield, inasmuch as that fault came into existence after the deposition of the Coal Measures. A shaft recently sunk has proved the correctness of the inference. The seam has been found to be well developed to the west of the fault, and a considerable addition has been made to our productive coalfields.

So much has been written about the range of the Devonshire disturbance under the south of England that I shall add no more than a brief comment on some of the evidence on which reliance has been placed. We have seen that there has been some post-Triassic movement along old lines of disturbance in North Wales and the Midlands and along the Malvern axis. It is suggestive therefore to find that in the region which we believe to be underlain by the east and west disturbance, east and west folding forms the dominant structure of the Secondary and Tertiary rocks.

The anticlines of the Vales of Pewsey and Wardour, the London syncline, the Wealden anticline, the Hampshire syncline, and the anticline of the Isles of Wight and Purbeck, not only lie in the range of the axis, but show an increasing intensity southwards, towards what we may suppose to have been the most active part of that axis. A similar structure prevails in the Oolitic rocks also. They too had been thrown into east and west folds before the Cretaceous period, and this earlier set of movements also grew in intensity towards the south. It would seem then at first sight that the structure of the later rocks gives an easy clue to the structure of the older rocks buried beneath them. This is by no means the case. That the movements manifested in the Oolitic and Cretaceous rocks followed the same general line as the older movement admits of little doubt, but that the later structures correspond in detail with the earlier is improbable.

A brief examination of the region where the Carboniferous rocks disappear under the Secondary formations will give the grounds for this statement. There we find that the Trias passes over the complicated flexures of the Mendip axis in undulations so gentle as to prove that those flexures

had been completed before it was deposited. Nor again do the members of the Oolitic group of the rocks cropping out in succession further east show any such folds as those visible in the Carboniferous, and it is not until we have passed over a considerable tract of Secondary rocks in which there are no signs of east and west folding that we reach the anticlines of the Vales of Pewsey and Wardour. Nor can we then fit these folds in the Cretaceous formation on to any visible axes in the Carboniferous rocks. In these circumstances it would be unjust to suppose that such synclines and anticlines as those of the London and Hampshire basins, or of the Weald, coincide with previously formed synclines and anticlines in the older rocks. They give a clue to the position of the old axis, but not necessarily to the details of its structure. Yet it is upon the determination of the position of the older anticlines and synclines, and of their intersection with the north and south disturbances, that we must depend for locating concealed coal-fields. So far but little has been done in the forty-eight years since the question was first mooted by Godwin-Austen. The existence of a coalfield in Kent has been proved, and what appears to be a prolongation of a disturbance from the Pas de Calais along the south-western side of it. The other borings which have reached the Palæozoic floor round London and at Harwich have thrown but little light on the details of its structure. By far the greater part of the ground remains yet to be explored.

In this brief review of the earth-movements of one period, as manifested in one small part of the globe, we have found reason to conclude that they were the result of compression and upheaval; that the crust yielded to the compression by overthrusting and buckling along certain belts; that these belts in the north of England and the Midlands ran for the most part north and south, diverging, however, to the south-west and to the south-east, while in the south of England they took an east and west direction and concentrated themselves along a belt of country which presents the phenomena of crushing on a stupendous scale. We have touched in two cases the flanks of a mountain-range, the Caledonian, which was built and ruined before the Carboniferous period; the Armorican, which was built after that period, and which, though it has stirred so recently as the late Tertiary period, and so energetically as to initiate the physical features and river-system of the south of England, yet expended the greater part of its energy before the Permian period. Lastly, we have found evidence, in the majority of cases, that the disturbances were but renewals of movement along lines of weakness long before established, and that in several cases there has been further renewal along the same lines during successive periods later than the one we have considered. With such a history before us, and with the knowledge that mountain-ranges have been built in other parts of the world by the upheaval of strata of almost recent date, we have more cause to wonder that the internal forces have left this quarter of the globe alone for so long, than reason to believe that they have ceased to exist. Changes of level, however, have taken place in comparatively recent times, and are now in progress. Though almost imperceptibly slow, they serve to remind us that a giant lies sleeping under our feet who has stretched his limbs in the past, and will stretch them again in the future. Nor in view of the fact that the structures I have described have only been revealed by the denudation of vast masses of strata does it seem unreasonable to suppose that they are deep-seated phenomena. The slow changes of level may be the outward manifestation of more complicated movements being in progress at a depth.

It is interesting to speculate on what appearance the globe would have presented had it not been enveloped in an atmosphere and covered for the most part with water. Owing to those circumstances it possesses the power of healing old wounds and burying old scars. In their absence we may suppose that the belts of crushing and buckling would have given rise to ridges growing in size at every renewal of movement, for they would have been neither levelled by denudation nor smoothed over by sedimentation. This globe, we may suppose, would have appeared to the inhabitants of another planet as being encompassed in a network, and we are prompted to ask whether our astronomers can distinguish in any other planet markings that may be attributable to this cause. I must remind you, however,

how much more remains to be done than I have been able to touch upon to-day. The map [appended to the address] represents one episode only in a long series of events, and a series of such maps would be required to illustrate the first appearance of lines of weakness in the earth's crust, the subsequent renewals of movement along those lines, and the formation of new lines in successive geological periods. With the case thus set out we shall be justified in appealing to the physicists for an explanation of the restlessness of this globe.

NOTES.

THE Antarctic relief ship *Terra Nova* arrived at Plymouth on Sunday night last, and afterwards left for Sheerness. It will be remembered that the *Terra Nova*, in company with the *Morning*, was engaged in the expedition for the relief of the *Discovery*, which was ice-bound in the Antarctic Sea. The two relief ships left Hobart together, and first encountered pack ice on January 4. They saw the mast-heads of the *Discovery* on January 8, and the crews of the three ships were engaged from that time until February 14 in blasting a passage through the 12 miles of ice which lay between the *Discovery* and open water. When they got within two miles of the *Discovery* the ice began to break up freely, and the task was quickly completed. The *Discovery*, having been supplied with coal by the *Terra Nova*, began her homeward journey, the two vessels during the early stages travelling in company. Subsequently the vessels parted owing to bad weather, but met again at the Auckland Isles. Thence they proceeded to Lyttelton and home.

THE first instalment of specimens collected by the National Antarctic Expedition in the *Discovery* has, according to the *Times*, arrived at the British Museum (Natural History). It consists of the collection of sealskins obtained by the expedition in the pack ice and in McMurdo Strait in the Polar summers of 1901-2, 1903-4. It is proposed to await the arrival of the *Discovery* before dealing with this instalment, which has been sent on ahead in order to ensure the proper preservation of the specimens; but the report which has been received with the collection indicates that the four species of true seals known to occur in the Antarctic are all represented. It is probable that the collection also contains one or more specimens of the elephant-seal from McMurdo Strait, a region where it was not hitherto known to exist. The remainder of the specimens collected by the expedition are coming home in the *Discovery*. On the arrival of the *Discovery*, the natural history specimens will be sent to the Cromwell Road Museum to be worked out, the trustees of the British Museum having, says the *Times*, undertaken the classification, description, and publication of the biological and geological results of the expedition.

WE much regret to have to announce the death, in his seventy-fourth year, of Dr. J. D. Everett, F.R.S., for thirty years professor of natural philosophy at Queen's College, Belfast.

WE note with great regret the death of the Rev. Dr. H. P. Gurney, principal of the Durham College of Science, Newcastle-upon-Tyne, which occurred on Saturday last through a fall while climbing in the Alps. Dr. Gurney, who did much to further the interests of science and education, became principal of the Durham College of Science in 1894, and was also professor of mathematics and lecturer in mineralogy in the same institution. In Newcastle he was a recognised leader in educational matters, and was a co-opted member of both the Newcastle and the Northumberland education committees, being particularly useful on the higher and other sub-committees.